

THE ACCOUNTING REVIEW

VOLUME 83, No. 2 MARCH 2008

BOK BAIK, BRUCE K. BILLINGS, and RICHARD M. MORTON

Reliability and Transparency of Non-GAAP Disclosures by Real Estate Investment Trusts (REITs)

ORIE E. BARRON, DONAL BYARD, and YONG YU

Earnings Surprises that Motivate Analysts to Reduce Average Forecast Error

BRUCE K. BEHN, JONG-HAG CHOI, and TONY KANG

Audit Quality and Properties of Analyst Earnings Forecasts

SCOTT B. JACKSON

The Effect of Firms' Depreciation Method Choice on Managers' Capital Investment Decisions

JOHN (XUEFENG) JIANG

Beating Earnings Benchmarks and the Cost of Debt

ELIZABETH K. KEATING, LINDA M. PARSONS, and ANDREA ALSTON ROBERTS

Misreporting Fundraising: How Do Nonprofit Organizations Account for Telemarketing Campaigns?

RYAN LAFOND and ROSS L. WATTS

The Information Role of Conservatism

ZOLTAN P. MATOLCSY and ANNE WYATT

The Association between Technological Conditions and the Market Value of Equity

WENDY M. WILSON

An Empirical Analysis of the Decline in the Information Content of Earnings Following Restatements

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The Accounting Review (ISSN 0001-4826 print and ISSN 1558-7967 online) is published six times a year in January, March, May, July, September, and November by the American Accounting Association, 5717 Bessie Drive, Sarasota, Florida 34233-2399. General subscriptions are available at the annual rates of \$340 for a print copy or at \$380 for a print and electronic copy. The member subscription rate for a hard-copy is \$30. Electronic access is included with membership dues. Periodical postage paid at Sarasota, Florida and additional mailing offices. **POSTMASTER:** Send address change to *The Accounting Review*, 5717 Bessie Drive, Sarasota, Florida 34233-2399.

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44

VOL. 83

MARCH 2008

No. 2

BOK BAIK, BRUCE K. BILLINGS, and RICHARD M. MORTON Reliability and Transparency of Non-GAAP Disclosures by Real Estate Investment Trusts (REITs)	271
ORIE E. BARRON, DONAL BYARD, and YONG YU Earnings Surprises that Motivate Analysts to Reduce Average Forecast Error	303
BRUCE K. BEHN, JONG-HAG CHOI, and TONY KANG Audit Quality and Properties of Analyst Earnings Forecasts	327
SCOTT B. JACKSON The Effect of Firms' Depreciation Method Choice on Managers' Capital Investment Decisions	351
JOHN (XUEFENG) JIANG Beating Earnings Benchmarks and the Cost of Debt	377
ELIZABETH K. KEATING, LINDA M. PARSONS, and ANDREA ALSTON ROBERTS Misreporting Fundraising: How Do Nonprofit Organizations Account for Telemarketing Campaigns?	417
RYAN LAFOND and ROSS L. WATTS The Information Role of Conservatism	447
ZOLTAN P. MATOLCSY and ANNE WYATT The Association between Technological Conditions and the Market Value of Equity	479
WENDY M. WILSON An Empirical Analysis of the Decline in the Information Content of Earnings Following Restatements	519

BOOK REVIEWS, Stephen A. Zeff, Editor

Tim Bush, <i>"Divided by Common Language," Where Economics Meets the Law: US versus non-US Financial Reporting Models</i>	GEORGE J. BENSTON	549
Kees Camfferman and Stephen A. Zeff, <i>Financial Reporting and Global Capital Markets—A History of the International Accounting Standards Committee, 1973–2000</i>	SHAHROKH M. SAUDAGARAN	552
Robert J. Kirsch, <i>The International Accounting Standards Committee: A Political History</i>	ANN TARCA	554
James Ohlson and Zhan Gao, <i>Earnings, Earnings Growth, and Value</i>	STEVEN HUDDART	556
Geoffrey Whittington, <i>Profitability, Accounting Theory and Methodology: The Selected Essays of Geoffrey Whittington</i>	PHILIP BROWN	557
Editorial Data		560
Editorial Policy and Style Information		561

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Reliability and Transparency of Non-GAAP Disclosures by Real Estate Investment Trusts (REITs)

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ABSTRACT: This paper examines whether industry efforts to decrease managerial discretion, increase uniformity, and improve transparency of a non-GAAP performance measure change voluntary disclosure and market perceptions. We find that the frequency of REITs meeting or beating analysts' expectations of funds from operations (FFO) decreases following explicit industry initiatives to discourage manipulation. Concurrent with this shift, we find that the information content of FFO to investors increased, particularly for firms reporting a reconciliation of FFO with GAAP earnings. We also examine firms in other industries to assess alternative explanations for these results, such as SEC intervention. Collectively, our findings suggest that industry guidance about non-GAAP performance curtailed managers' opportunistic reporting. Furthermore, the market response to FFO is consistent with investors perceiving less manipulation and greater reliability. Our evidence also supports the SEC's subsequent requirement that all non-GAAP disclosures be reconciled to the nearest GAAP measures.

Keywords: *funds from operations; REIT; non-GAAP measures; analysts' expectations; stock market reaction.*

Data Availability: *Data used in this study are available from public sources.*

We appreciate the helpful comments of Anwer Ahmed, Frank Heflin, Carol Marquardt, Steve Rock, Phil Shane, Kumar Sivakumar, two anonymous reviewers, workshop participants at the University of Colorado, and participants at the 2006 FARS Conference and 2006 AAA Annual Meeting. We also thank I/B/E/S, a service of Thomson Financial, for providing data as part of a broad academic program to encourage earnings expectation research.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted May 2006
Accepted September 2007

I. INTRODUCTION

In recent years, it has become common for firms to disclose alternative measures of performance that depart from GAAP. Increasing concern over the reliability of these disclosures led the Securities and Exchange Commission (SEC) to enact Regulation G (effective March 28, 2003) requiring firms to include with the disclosure of any non-GAAP measure a reconciliation to the nearest GAAP measure. Recent evidence suggests that this SEC mandate effectively discouraged voluntary reporting of non-GAAP performance measures (Heflin and Hsu 2007; Entwistle et al. 2006; Marques 2006). In contrast to these studies, we examine the role of industry guidance aimed at promoting and shaping voluntary disclosure of a particular non-GAAP performance measure. Specifically, we study the disclosure of funds from operations (FFO) in the real estate investment trust (REIT) industry, and investigate whether changes prescribed by the main industry trade association to make FFO less discretionary and more transparent discouraged manipulation and enhanced market perceptions. This allows us to observe the effects of oversight on non-GAAP disclosure quality in an uncensored sample.¹

While FFO falls under the broad heading of non-GAAP performance measurement, it is unique in several important respects. FFO is the result of a concerted industry-wide effort to develop a single best alternative to GAAP-based earnings. Within the REIT industry, FFO is the performance metric universally disclosed by firms and closely followed by market participants. Historically, financial analysts following REITs have forecasted FFO rather than earnings per share (EPS), and only recently have they begun to publish both FFO and EPS forecasts. In contrast to other non-GAAP measures, FFO's prominence makes it difficult for REITs *not* to report this amount. While discretion has been an issue with all non-GAAP performance measurement, the REIT industry proactively sought to encourage greater uniformity in FFO reporting with fuller disclosure prior to SEC regulation. Unlike the SEC's actions, which discouraged other non-GAAP disclosures, this industry guidance sought to advance a supplementary measure via self-regulation.

Spearheading REIT industry efforts to promote FFO reporting is the National Association of Real Estate Investment Trusts (NAREIT). This organization, primarily sponsored by member REITs, acts on the industry's behalf before policymakers, the media, and the capital markets, but has no real authority over REIT reporting. Since 1991 when they first proposed FFO as a supplementary performance measure, NAREIT made several attempts to exercise its influence over REITs by making specific recommendations that would result in greater uniformity and transparency in FFO reporting. The most notable event was in 1999, when NAREIT issued a National Policy Bulletin, effective January 1, 2000. This Bulletin modified the definition of FFO to reduce discretion and reinforce NAREIT's intent that FFO be constructed from financial data measured in conformity with generally accepted accounting principles (GAAP).² An express purpose of the Bulletin was to increase the credibility of FFO in the financial markets. In light of the broad-based support for the 1999

¹ Changes in the population of firms disclosing non-GAAP performance after SEC Regulation G makes it difficult for other studies to evaluate its effect on reporting reliability and value relevance. The full sample of interest cannot be observed because firms opt out and do not disclose. Heflin and Hsu (2007) report that over 30 percent of their sample firms that disclosed non-GAAP earnings move toward more GAAP reporting after Regulation G. Alternatively, we find that of 130 REITs disclosing FFO just prior to concerted industry guidance, only four stopped reporting FFO following increased oversight. One of the four firms liquidated, and a second firm was exiting the REIT industry.

² NAREIT clarified the definition in 1995 and 1999, but according to NAREIT, the 1999 National Policy Bulletin was their first major effort to address the issue of uniformity. NAREIT also issued a white paper regarding FFO in April 2002, in response to SFAS No. 144, *Accounting for the Impairment or Disposal of Long-Lived Assets*.

Policy Bulletin, we investigate whether the extent of FFO manipulation diminished dramatically following the effective date of this Policy Bulletin.

Results are consistent with a decrease in managerial discretion and an increase in uniformity regarding FFO following the 1999 National Policy Bulletin. In particular, we find that the frequency of firms meeting or beating FFO expectations decreases significantly after 1999, coinciding with NAREIT efforts to discourage manipulation and improve transparency of the non-GAAP measure.³ We also examine the earnings surprise distributions of other (non-REIT) firms tracked by I/B/E/S in these years and find no evidence of a similar decrease. Further, we examine a random sample of REITs and find that the extent of discretionary nonrecurring items included in FFO significantly decreased after 1999, and this change was most dramatic for FFO-increasing items. These results suggest that to the extent managers used FFO to inflate reported performance, such manipulation abated in the 2000–2003 period.

We next consider the potential effect of industry self-regulation on investor perceptions. If investors perceive FFO to be a more credible measure of performance, then we expect it would be more useful for firm valuation. We assess whether the market's response to unexpected FFO increased following NAREIT 1999 initiatives. Consistent with greater reliability leading to greater usefulness, we find that the relation between unexpected FFO and announcement-period returns increases concurrent with the release of NAREIT's recommendations. We do not observe a similar increase in the information content of non-GAAP disclosures for non-REIT firms at that same time.

Finally, we assess the merits of greater reporting transparency to investors by testing whether the information content of FFO increases for firms providing a reconciliation of FFO with GAAP earnings. We find that the FFO-return relation is stronger for those firms voluntarily reconciling FFO to GAAP EPS, consistent with greater transparency contributing to greater reliability. This effect holds controlling for SEC changes and firm characteristics related to disclosure choice.

Our study makes several important contributions to the literature. First, we provide additional evidence consistent with the manipulation of non-GAAP performance measures for the purpose of meeting market expectations. Second, and more importantly, we demonstrate how industry oversight can play a critical role in mitigating opportunistic reporting. Finally, we find differences in investor responses to a non-GAAP performance measure in situations where its perceived reliability is expected to vary. Specifically, during a period when managers appear to exercise less opportunistic discretion and when firms provide more transparent disclosure, we find the value relevance of FFO to be enhanced. While our study corroborates the incremental information content of non-GAAP measures documented in other studies, our findings concerning the interactive role of nonregulatory reporting oversight distinguish our study, making it of particular interest to standard setters.

We also acknowledge several limitations. First, our results pertain to a homogeneous sample of REITs and may not generalize to firms in other industries. Second, we cannot directly observe FFO manipulation, thus our inferences concerning managerial opportunism depend on how well our analyses of reported FFO identify this behavior. Last, our assessment of the information content of FFO depends on our ability to estimate unexpected returns conditional on the FFO-returns-generating process.

³ Hefflin and Hsu (2007) find a decrease in the probability of meeting or beating analysts' forecasts for non-GAAP disclosers following the SEC's non-GAAP disclosure regulations, effective March 2003. To distinguish NAREIT's influence from the effects of SEC monitoring and regulation, we perform additional temporal and control sample analyses, as described later in the paper.

In the remainder of our paper, Section II briefly reviews other studies investigating non-GAAP performance measures, as well as a background of FFO reporting in the REIT industry. Section III discusses our hypotheses, and Section IV presents our research design and results. Section V presents our conclusions.

II. BACKGROUND

Non-GAAP Earnings Research

Bradshaw and Sloan (2002), among the first to investigate street earnings, present evidence suggesting that over time managers have increasingly excluded transitory expenses from GAAP earnings in arriving at street earnings. This pattern could reflect either management incentives to disclose a more informative measure of firm performance or management incentives to manipulate performance measures for opportunistic reasons. Supporting the former, Bradshaw and Sloan (2002) and Brown and Sivakumar (2003) find that street earnings are more closely related to stock returns than are GAAP earnings and that the relation is increasing over time. Bhattacharya et al. (2003) find similar results using pro forma numbers from press releases. Lougee and Marquardt (2004) further find an inverse relation between the incremental information content of pro forma earnings and GAAP earnings informativeness.

Other evidence suggests that pro forma earnings may reflect earnings management. Bhattacharya et al. (2004) note that firms selectively exclude expenses from earnings from year to year and those exclusions assist in meeting/beating analysts' forecasts (also see Doyle and Soliman 2005). Doyle et al. (2003) find that excluded items have significant predictive ability for future operating cash flows and for future abnormal returns, suggesting that investors are misled by the adjustments to GAAP earnings.⁴ Frankel et al. (2007) find that exclusions necessary to achieve earnings benchmarks are more likely when boards are less independent. Lougee and Marquardt (2004) find that firms are more likely to report pro forma earnings when their GAAP earnings result in a negative surprise, and they find weak evidence of predictable future stock returns in these cases. Likewise, Bowen et al. (2005) find emphasis of the more favorable earnings metric (pro forma versus GAAP) in press releases, and greater emphasis is associated with a stronger market reaction.⁵ In summary, prior research also supports the notion that managers selectively define and disclose alternative earnings measures to meet certain performance benchmarks with the objective of fooling investors.

The Securities and Exchange Commission (SEC) shared these concerns.⁶ They issued cautionary advice in 2002 and implemented rules imposing stricter controls over non-GAAP disclosures effective March 28, 2003. Bowen et al. (2005), Marques (2006), Kolev et al. (2008), Entwistle et al. (2006), and Heflin and Hsu (2007) investigate the effects of these SEC actions. They find a decline in management emphasis of pro forma (GAAP) earnings in quarterly earnings announcements and press releases and a decline in the frequency of disclosing pro forma earnings. Pro forma earnings that were disclosed after these SEC

⁴ In contrast, Gu and Chen (2004) conclude that analysts exclude less persistent "nonrecurring" items, and they find no evidence of future abnormal returns associated with the excluded items.

⁵ Alternatively, Bhattacharya et al. (2003) suggest that investors and analysts are somewhat skeptical of positive pro forma news in the face of negative GAAP news.

⁶ Section 401(b) of the Sarbanes-Oxley Act of 2002 directs the SEC to establish rules regulating non-GAAP earnings disclosures. The SEC subsequently issued Regulation G, which requires a reconciliation of disclosed non-GAAP measures to the closest GAAP measure, and amended Regulations S-K and S-B concerning the use of non-GAAP measures in SEC filings. Those rules require that any non-GAAP disclosure be accompanied by the most directly comparable GAAP measure, that the disclosure include a reconciliation of the non-GAAP to comparable GAAP measure, and that the non-GAAP measure not mislead investors.

actions were less likely to meet or beat analysts' forecasts, and tended to exclude more transitory items.

Zhang and Zheng (2004) find predictable stock returns (price reversals) for firms reporting greater differences between pro forma and GAAP earnings, suggesting that investors may give undue attention to pro forma earnings. They also find that firms voluntarily disclosing high-quality reconciliations prior to Regulation G have less mispricing. Subsequent to Regulation G, they find no evidence of mispricing in their sample.⁷

FFO Background and Research

NAREIT's efforts to promote FFO as an alternative to EPS are primarily based on its objection to historical cost depreciation. NAREIT argues that because the value of real estate generally does not decrease in a predictable manner over time, performance comparisons among REITs based on GAAP earnings are "less than satisfactory" (NAREIT 2002). In addition to adding back depreciation, NAREIT proposed a number of other adjustments with its initial definition of FFO in 1991: "Funds from operations means net income (computed in accordance with generally accepted accounting principles), excluding gains (or losses) from debt restructuring and sale of property, plus depreciation and amortization, and after adjustment for unconsolidated partnerships and joint ventures" (NAREIT 1991). Further guidance by NAREIT in 1995 suggested that only depreciation and amortization on assets unique to the real estate industry should be added back, that significant nonrecurring items should be excluded from FFO, and that firms should include a detailed reconciliation of FFO to GAAP earnings.

Given claims that FFO is a superior performance measure, several research studies in the later 1990s compare the relative usefulness of FFO to GAAP earnings. The evidence is mixed. During 1991–1995, Fields et al. (1998) find that FFO is more strongly associated with year-ahead FFO and operating cash flows, while GAAP earnings is more strongly associated with year-ahead GAAP earnings. They also find that firms that include straight-line rents in FFO are on average more cash-constrained and younger than those firms that exclude straight-line rents from FFO.⁸ This suggests that firms were exercising discretion by using a more aggressive definition of FFO (i.e., including the straight-line accrual) when they had need to access capital markets. During 1994–1996, Vincent (1999) finds that both FFO and GAAP earnings provide incremental information related to stock returns under certain model specifications.⁹ While these studies document the value relevance of FFO and suggest that FFO provides incremental information beyond GAAP earnings for investors, neither Fields et al. (1998) nor Vincent (1999) concludes that FFO is a superior

⁷ Results in Zhang and Zheng (2004) are consistent with a number of scenarios regarding how investors actually use reconciliation information. For example, investors may naively price non-GAAP disclosures; however, firms with higher quality reconciliations are not attempting to mislead investors and therefore exhibit less price reversal. Another scenario could be that all firms attempt to mislead through non-GAAP disclosures; however, investors see through that attempt via higher quality reconciliations and lower their reliance on non-GAAP disclosures for those firms while misplacing greater reliance on non-GAAP disclosures for lower quality reconcilers.

⁸ Multi-period leases often contain a clause that escalates rent in later years of the lease. In certain situations, GAAP requires the straight-line method to recognize rent revenue over the lease term, accelerating revenue recognition. "Straight line rents" represent the difference between amounts recognized under the straight-line method and amounts currently due. For REITs that are growing, the accelerated revenue recognized in the early years of newer leases more than offsets the reduced revenue recognized in the later years of the older leases (Fields et al. 1998).

⁹ For announcement-window returns (days -1 to +1), Vincent (1999) finds incremental information for FFO and GAAP earnings when the model specification follows Biddle et al. (1995) by including current and lagged FFO and current and lagged GAAP earnings in the model. For long-window returns, Vincent (1999) finds that only GAAP earnings have an incremental relation.

performance measure to GAAP earnings. This is noteworthy in that these results are at odds with the evidence discussed earlier suggesting that street and pro forma earnings are more highly correlated with stock market returns than GAAP earnings. Potentially, the weak relation between FFO forecast errors and stock returns documented in Vincent (1999) reflects market skepticism concerning the reliability of FFO during this time.

By 1998, NAREIT noted the considerable discretion being exercised by firms in calculating FFO. The exclusion of nonrecurring items from FFO recommended in 1995 created an opportunity for increased managerial discretion in that the nonrecurring classification was not being consistently applied across firms or time. This resulted in FFO measures that lacked uniformity and reliability. The financial analyst community grew increasingly skeptical of FFO and openly expressed concern over inconsistencies in the calculation of FFO that allowed firms to inflate reported results (Martinez 1998; Salomon Smith Barney 2000).

At this same time, REITs were eager to improve their standing in the capital markets. Rapid growth and strong returns averaging about 16 percent annually through the mid-1990s brought greater attention to (and scrutiny of) the REIT industry from the investment community. However, poor stock performance in 1998 (−18 percent) and 1999 (−6 percent) diminished the luster of REIT stocks (NAREIT 2000). NAREIT lobbied for the inclusion of REITs in the S&P 500 to broaden their exposure to potential investors.¹⁰ However, FFO was a stumbling block in this effort because it was seen as an unconventional measure of performance that differentiated REITs from other industries included in the index (Wright 2003).

In light of these events, NAREIT established a Best Financial Practices Council comprised of industry executives, analysts, institutional investors, auditors, and other industry participants to enhance the quality and effectiveness of financial reporting. The convening of this council represented NAREIT's first major effort to address the issue of uniformity in FFO.¹¹ The result of this effort was a National Policy Bulletin, effective January 1, 2000, which formally approved the recommendations of the best practices council to improve REIT reporting. This bulletin had two main features that have the potential to reduce managerial discretion and enhance the uniformity and reliability of FFO. First, the definition of FFO was modified to include nonrecurring operating results, except for extraordinary gains and losses as defined by GAAP and gains and losses from sale of depreciable operating property.¹² Second, the bulletin reinforced NAREIT's desire to curtail opportunism and encourage uniformity by emphasizing that "The underlying premise of the definition of FFO [is] not to sanction deviations from GAAP" (NAREIT 1999). Rather, the bulletin emphasized that all adjustments to GAAP earnings when calculating FFO should be measured in accordance with GAAP. In general, NAREIT's objective was to reinforce the role of GAAP earnings for comparison with other industries and clarify the definition of FFO as a supplementary measure within the industry.

Despite NAREIT's efforts to make FFO more uniform, not all firms complied, and the FFO debate persisted. Lynn Turner, former chief accountant for the SEC, commented that

¹⁰ Because REITs are required by law to distribute at least 90 percent of their earnings to shareholders, capital market access is critical to fund future growth.

¹¹ We appreciate helpful discussions with George Yungmann, Vice President of Financial Standards for NAREIT, regarding the importance and purpose of the Best Financial Practices Council and resultant 1999 National Policy Bulletin.

¹² The Bulletin listed specific examples of nonrecurring items, such as gains (losses) on derivative and hedging arrangements, costs of abandoned transactions, provisions for potential losses other than for depreciable operating property, merger integration and REIT conversion costs, costs of unusual compensation or severance arrangements, and debt restructuring costs except those considered extraordinary under GAAP.

“with FFO there is no common definition, and people pick and choose the numbers they want to use to calculate FFO” (Starkman and Weil 2001). Some analysts opted to forecast EPS in addition to FFO (Smith 2001), perceiving GAAP earnings as less discretionary than an unaudited FFO measure. Interestingly, analysts were not necessarily abandoning FFO as the best supplementary performance measure for REITs, as much as they were stressing the importance of EPS as a universal measure across industries.

III. HYPOTHESES

The evolution of FFO provides a rich setting for the study of opportunistic disclosure and investor response to those disclosures for several reasons. First, the prominence of FFO in the popular press and as an industry-specific measure of performance among REITs suggests that managers likely perceive FFO manipulation as a possible means of influencing decision makers, particularly investors.¹³ Furthermore, managers have wide latitude to define FFO as they see fit. Fields et al. (1998) suggest that REIT managers exercise discretion in defining FFO prior to accessing capital markets. Similarly, we expect that managers may opportunistically define FFO in order to achieve certain performance objectives, including meeting or beating analyst forecasts. Degeorge et al. (1999) provide evidence that managers consider the analyst forecast as an important performance benchmark, and Brown and Caylor (2005) suggest that the analyst forecast has become the primary benchmark for managers and investors since the mid-1990s.¹⁴

Despite the economic motivation for firms to manipulate FFO, other capital market forces may have had a disciplining effect on REITs. Heightened investor and analyst skepticism in the late-1990s raised doubts about the reliability of FFO, which NAREIT recognized as a potential threat to the longer-term interests of its member firms. Insulated from the short-term reporting pressures facing individual managers, NAREIT provided direction aimed at restoring credibility to FFO reporting. Given the industry-wide emphasis on NAREIT's 1999 modified definition of FFO with transparent disclosure, we expect that those firms failing to comply would likely suffer from an adverse selection problem, whereby analysts and investors might be apt to discount their reported FFO.^{15,16} Although member REITs sponsor NAREIT, it is an empirical question as to whether individual firms voluntarily adopted the organization's recommendations and FFO manipulation decreased. This leads to our first hypothesis, stated in the alternative form:

H1: The incidence of meeting or beating analysts' FFO forecasts declined subsequent to NAREIT's efforts in 1999 to improve the reliability of FFO.

We then extend our analysis to examine investor pricing of FFO information. We expect that NAREIT's recommendations for a more uniform definition of FFO should discourage managerial manipulation and produce a more consistent and comparable measure. As a

¹³ For example, REIT stock prices are typically expressed as multiples of FFO rather than EPS in the *Wall Street Journal* (see Starkman 2000; Opdyke 1996).

¹⁴ Brown and Caylor (2005) suggest that the incidence of meeting or beating analysts' forecasts of EPS has increased in every year over the 1996–2002 period, consistent with strong economic incentives.

¹⁵ The importance of analyst monitoring can be seen in the response to Rouse's third quarter, 2001, earnings release. Legg Mason Wood Walker Inc. downgraded Rouse after reporting FFO that excluded certain employment-related expenses, such as an executive signing bonus, which the investment bank considered recurring costs (Jones 2002). This situation illustrates both the freedom of individual REITs to deviate from NAREIT's 1999 definition, and the potential consequences.

¹⁶ Bartov et al. (2002) find a smaller market reward for meeting/beating expectations when earnings management is suspected, consistent with investors responding skeptically and discounting the earnings information.

result, we expect that investors will perceive FFO to be a more reliable measure of underlying performance increasing its usefulness for firm valuation (Teoh and Wong 1993; Collins and Salatka 1993). Thus, our second hypothesis follows:

H2: The stock market reaction to unexpected FFO at the announcement date increased subsequent to NAREIT's efforts in 1999 to improve the reliability of FFO.¹⁷

We believe that evidence consistent with a decrease in opportunistic reporting of FFO (H1) and a corresponding increase in investor reliance on FFO (H2) would suggest that investors recognize the potential for opportunism in a highly discretionary non-GAAP performance measure, and prices reflect changes in the perceived reliability.¹⁸ Further, these results would not only contribute to existing literature suggesting that non-GAAP performance measures are relevant to investors, but also highlight the importance of uniformity and transparency in reporting those measures. An alternative prediction might be that less discretion in performance measurement will lead to a decrease in relevance if it hinders managers' ability to disclose private information to investors. However, given the growing skepticism of FFO among analysts at that time, we believe the benefits of greater reliability will dominate.

SEC rules effective in 2003 specify that all firms disclosing non-GAAP performance measures must also provide a reconciliation of that measure with the most directly related GAAP measure. This requirement reflects the SEC's concerns about non-GAAP measures misleading investors and their desire to increase transparency and reduce information asymmetry. In the 1990s, many REITs voluntarily reconciled FFO to GAAP earnings following NAREIT's recommendation.¹⁹ This provides us the opportunity to test the assertion that these reconciliations increase the transparency and credibility of FFO reporting, resulting in greater usefulness of FFO for firm valuation. Such evidence would substantiate the SEC's mandate for all firms to reconcile reported non-GAAP and GAAP numbers. Therefore, our third hypothesis states:

H3: The stock market reaction to unexpected FFO at the announcement date is greater for firms providing a reconciliation of FFO to GAAP earnings.

¹⁷ NAREIT's recommendation to include nonrecurring items in FFO after 1999 would likely work in favor of the null hypothesis. The coefficient mapping nonrecurring items to stock returns is expected to be smaller than the coefficient mapping recurring items to stock price. This would work against us finding an increase in the market response to FFO attributable to increased reliability.

¹⁸ Although, we describe a scenario in which investors efficiently respond to an improved FFO measure, we recognize the possibility of observing an increase in investor response to FFO regardless of the extent of manipulation. In other words, investors may inappropriately attach more weight to FFO following industry guidance, expecting it to be a more reliable measure, even if there is no real change in the underlying reliability. Such a scenario would require some degree of market inefficiency. Although we do not assume market inefficiency at this point, we investigate the possibility that FFO is mispriced later in the paper.

¹⁹ See Healy and Palepu (2001) for a comprehensive review and discussion of the voluntary disclosure literature. In our setting, pressure to conform to the industry standard appears to be evident. About 54 percent of our firms report a reconciliation in 1995. The frequency grows steadily to about 96 percent in 2000 and remains fairly stable until mandatory disclosure occurs in 2003. Once a firm provides a reconciliation, it generally continues the practice, suggesting that the decision is not driven by short-term (temporary) motives or incentives. We identify only seven firms (0.7 percent of our sample) that discontinued reporting a reconciliation. Three of those firms delisted, and the remaining firms resumed the practice in later years.

IV. DATA, RESEARCH DESIGN, AND RESULTS

Sample Selection

Our initial sample includes all firms listed on the I/B/E/S detail file with non-missing FFO forecasts and actual FFO beginning in 1995, when FFO first appears in I/B/E/S, and ending in 2003. We confirm that this sample includes strictly real estate firms (SIC codes 6798, 6799). Our primary measure of interest is the FFO forecast error, defined as actual FFO less the consensus forecast of FFO.²⁰ The consensus forecast is estimated as the mean of each analyst's final FFO forecast prior to the announcement date. To eliminate outlier effects, we delete firm-year observations for which the price-deflated forecast error is above the 99th percentile or below the 1st percentile. Our final sample consists of 1,205 firm-year observations for the period 1995 to 2003. For tests of incremental relevance, we also obtain EPS forecasts for our sample where available from the I/B/E/S detail file. Reconciliations of FFO to GAAP earnings are collected from annual reports and press releases for the sample years. All other accounting data is taken from Compustat, and stock returns are obtained from the Center for Research in Security Prices (CRSP) database.

Table 1 provides summary statistics of FFO forecast errors for the sample. The number of observations ranges from 113 in 1995 to 145 in 1997 and 2003. The distribution of FFO forecast errors is skewed to the left (mean and median of -4.6 and 0.2 cents per share, respectively). Interestingly, the median surprise decreases over the sample period, shifting from a positive to negative forecast error. The results based on price-deflated forecast errors are qualitatively similar. In untabulated statistics we find that the mean (median) number of analysts forecasting FFO for a given firm is 7.9 (7.0) and generally increases over the sample period. However, the mean (median) number of analysts forecasting EPS, available in later sample years, is only 3.0 (2.0) for a given firm, suggesting that analysts continue to view FFO as the primary performance measure.

Research Design and Results

Our study focuses on the ability of NAREIT to influence FFO reporting through its National Policy Bulletin, effective January 1, 2000. Accordingly, we divide our sample period into two subperiods, 1995–1999 and 2000–2003, and conduct two tests to determine whether the incidence of meeting or beating analysts' FFO forecasts declined in 2000.²¹

In developing our first hypothesis we implicitly assume that REIT managers believe FFO can significantly influence investor perceptions. If this were not the case, then they would not necessarily be motivated to manipulate FFO to meet analyst expectations.²² Although the popular business press suggests that FFO is closely watched by investors, and prior literature indicates that FFO is significantly related to stock prices, it is important that

²⁰ With other studies of non-GAAP earnings there has been some concern that analyst tracking service data does not correspond to pro forma numbers actually reported by management (Bhattacharya et al. 2003), making it difficult to study management behavior. In the REIT industry, we are much more confident that analysts and managers are focused on the same underlying construct. Notwithstanding, we examine a random sample of 62 REITs and trace the actual FFO amount available from I/B/E/S to the firm's press release. We find that the press release amount differs for only eight firms (13 percent). Three of these were within \$0.01 per share, and the mean difference of \$-0.004 is insignificant. Thus, the resultant forecast error should be a suitable proxy for unexpected FFO.

²¹ Since we test whether firms modify their definition of FFO immediately after the January 1, 2000 effective date, firms with fiscal years ending January 31 through December 31, 2000 are included in 2000–2003. We group REITs based on fiscal year-end in all sample years and apply the same rule to non-REIT firms in later comparative analysis.

²² Managers might have incentives to manipulate FFO for compensation reasons, but it seems unlikely that analyst forecasts would be the relevant benchmark in that case.

TABLE 1
Descriptive Statistics for FFO Forecast Errors

FFO Forecast Errors by Year								
Year	Forecast Error in Dollars				Forecast Error Deflated by Price			
	n	Mean	Median	Std.	n	Mean	Median	Std.
1995	113	-0.000	0.003	0.086	85	0.001	0.000	0.002
1996	141	-0.012	0.005	0.155	107	-0.000	0.000	0.005
1997	145	-0.001	0.002	0.114	119	0.000	0.000	0.002
1998	143	-0.091	0.001	0.289	114	-0.005	0.000	0.011
1999	130	-0.178	0.005	0.961	113	-0.001	0.000	0.007
2000	127	-0.029	0.003	0.147	113	-0.001	0.000	0.006
2001	131	-0.071	0.001	0.541	115	-0.001	0.000	0.005
2002	130	-0.016	-0.005	0.088	116	-0.001	-0.000	0.005
2003	145	-0.020	-0.006	0.165	119	-0.001	-0.000	0.005
1995-2003	1,205	-0.046	0.002	0.393	1,001	-0.001	0.000	0.006

This table reports mean and median funds from operations (FFO) forecast errors for 1,205 firm-year observations of real estate investment trust (REIT) firms for 1995-2003.

FFO forecast error in dollars = actual FFO minus the consensus forecast of FFO;

FFO forecast error deflated by price = (actual FFO - consensus FFO)/one-year lagged price;

Consensus forecasts = the average of each individual analyst's last forecast prior to the announcement date;

Std. = the standard deviation of the forecast errors; and

n = the number of firm-year observations.

we first demonstrate the value relevance of FFO for our sample. Because FFO is obtained by adjusting GAAP earnings, the incremental value relevance of FFO and GAAP earnings is also a concern. If investors can easily construct FFO from existing GAAP disclosures, attempts to define FFO opportunistically should be obvious. In addition to documenting the incremental value relevance of FFO, we also assess the ability to construct FFO from available annual report data for a subset of our sample.

NAREIT's primary objective in creating FFO was to encourage an alternative operating performance measure for computing price multiples (NAREIT 2002). Rather than comparing REITs on the basis of price-to-earnings ratios, NAREIT envisioned the price-to-FFO ratio as a more meaningful measure. Given NAREIT's focus on a basic capitalization ratio, we first report in Panel A of Table 2 the correlations between year-end stock prices, reported FFO, and reported earnings for our two sample subperiods. We find that both FFO and EPS are highly correlated with stock prices in both subperiods. The partial correlations reveal that each performance measure has an incremental relation with price after controlling for the other metric. Furthermore, the incremental price-to-FFO relation increases after 1999, while the incremental price-to-earnings ratio weakens. We also note that FFO and EPS are significantly correlated with each other and the relation strengthens after 1999. Thus, while FFO and EPS are related, each captures some unique information associated with prices.

To further evaluate the incremental value relevance of FFO for our sample, we regress annual market-adjusted returns on unexpected FFO (*UFFO*) and unexpected EPS (*UEPS*), where the unexpected measures are defined as actual minus the consensus forecast. Where EPS forecasts are not available, we use the change in annual EPS before extraordinary

TABLE 2
Value Relevance of FFO

Panel A: Correlation and Partial Correlation Coefficients

	$\rho_{Price, FFO}$	$\rho_{Price, EPS}$	$\rho_{Price, FFO EPS}$	$\rho_{Price, EPS FFO}$	$\rho_{FFO, EPS}$	<i>n</i>
1995–1999						
Pearson	0.432	0.536	0.183	0.369	0.613	555
Spearman	0.521	0.599	0.295	0.444	0.538	
2000–2003						
Pearson	0.687	0.591	0.479	0.233	0.681	476
Spearman	0.778	0.668	0.597	0.325	0.663	

Panel B: Annual Returns Regressed on *U*FFO and *U*EPS

	Intercept	<i>U</i> FFO	<i>U</i> EPS	Adjusted R ²	F-value	<i>n</i>
Predicted Sign		+	+			
1995–1999	−0.15 (<0.01)	3.68 (<0.01)	1.22 (<0.01)	0.05	10.33***	365
2000–2003	0.29 (<0.01)	11.98 (<0.01)	0.84 (<0.01)	0.06	15.35**	430

, * Denote significance at the 10, 5, and 1 percent levels, respectively.

n = the number of firm-year observations.

Panel A reports Pearson and Spearman correlation and partial correlation coefficients for stock price, funds from operations (FFO), and earnings per share (EPS). For example, $\rho_{Price, FFO}$ = the correlation between *Price* and *FFO*, and $\rho_{Price, FFO|EPS}$ = the partial correlation between *Price* and *FFO* after controlling for the correlation between *Price* and *EPS*. All correlations are significant at < 0.001 .

Panel B presents results of regressing annual buy-and-hold market-adjusted returns on unexpected FFO and EPS. Buy-and-hold market-adjusted returns are based on the value-weighted market portfolio and are measured over the 12-month period beginning three months after the end of the prior fiscal year. *U*FFO = unexpected FFO measured as (actual FFO – consensus FFO)/one-year lagged price; and *U*EPS = scaled unexpected EPS, measured as (actual EPS – consensus EPS forecast)/one-year lagged price. Where EPS forecasts are not available, *U*EPS = the scaled annual change in EPS before extraordinary items; and Consensus forecasts = the average of each individual analyst's last forecast prior to the announcement date.

Regressions are estimated using White's (1980) correction for heteroscedasticity. The numbers in parenthesis denote *p*-values for a one-tailed test when the sign is predicted, and a two-tailed test otherwise.

items as the measure of *U*EPS.²³ Our use of annual returns assesses the extent to which unexpected FFO and EPS reflect value-relevant information, while not isolating either as the original source of that information. The return relations also indicate that both FFO and EPS had incremental value relevance in the 1995–1999 and 2000–2003 periods.²⁴ These results further suggest to us that FFO had a distinct relation with stock prices in both subperiods, supporting our belief that REIT managers had an economic incentive to manage FFO.

We also consider the uniqueness of FFO from purely a financial statement perspective. In other words, to what extent is FFO simply an aggregation of existing annual report data? Using the estimation model in Vincent (1999), we construct estimates of FFO from GAAP

²³ We obtain from I/B/E/S 194 firm-year observations with both FFO and EPS forecasts. To assess the sensitivity of our results in Panel B of Table 2, we estimate the regression for this subset of data, and find significance for *U*FFO but not *U*EPS in the 2000–2003 period.

²⁴ In untabulated results, we estimate separate regressions of annual returns on *U*FFO and annual returns on *U*EPS. We find a significant increase in value-relevance for *U*FFO in 2000–2003 but not for *U*EPS.

financial statements for a random sample of 51 firms that disclose details of their calculation.²⁵ We restrict the analysis to 1998, which coincides with growing analyst criticism in the late-1990s regarding FFO measurement. Whereas Vincent (1999) reports a mean (median) estimation error as a percentage of reported FFO of -0.48 percent (-0.45 percent) in 1996, we find a mean (median) percentage error of 8.2 percent (6.6 percent), consistent with a much larger positive FFO bias (i.e., model underestimation) in 1998.²⁶ Differences between our estimates and reported amounts attest to the diversity and complexity of FFO reporting, which cannot be captured by a single aggregation model. Furthermore, many of the actual adjustments firms make to arrive at FFO, such as a specific nonrecurring loss or amortization of selected lease costs, are not itemized elsewhere in the financial statements making it impossible to reconstruct FFO. The Appendix provides two sample-firm reconciliations of FFO to GAAP net income as found in the annual reports.

Testing for Decline in Frequency of Meeting/Beating Analyst FFO Forecast

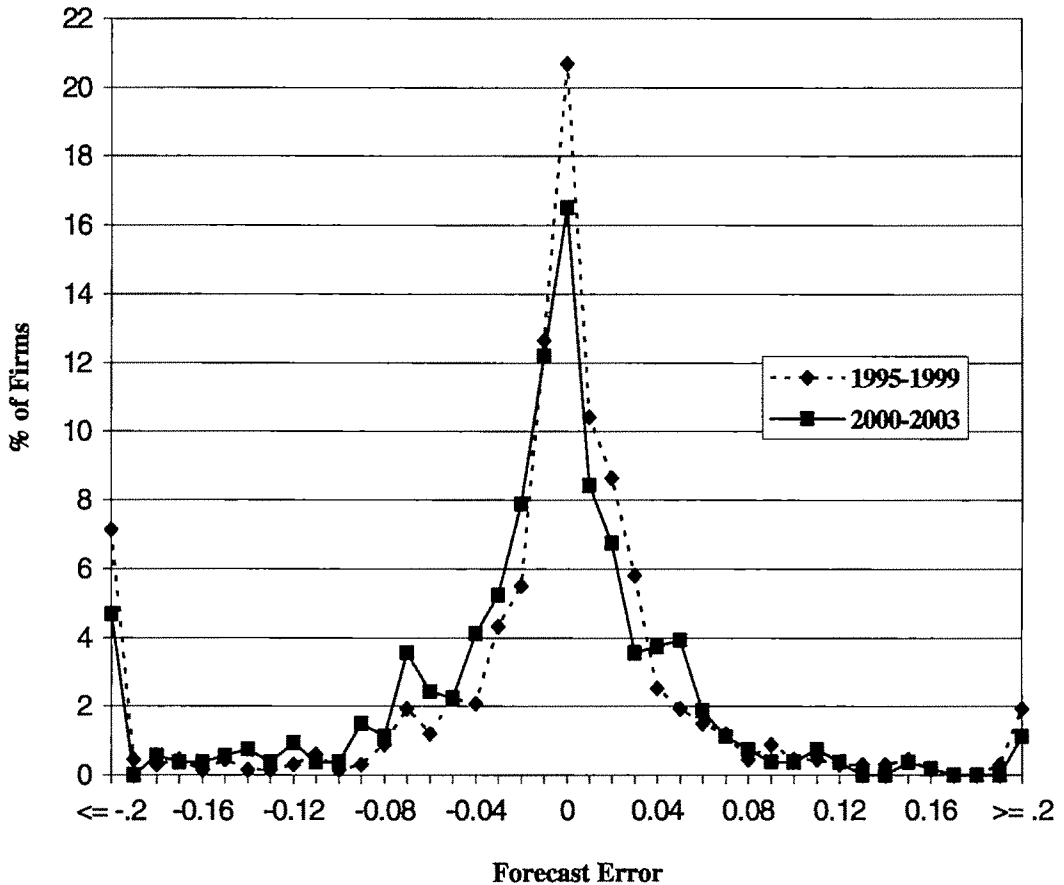
Based on these results we consider our first hypothesis. Consistent with prior studies of opportunistic reporting of non-GAAP measures (Bhattacharya et al. 2004; Doyle and Soliman 2005; Frankel et al. 2007), we examine the incidence of meeting or beating expectations. We first test for differences in the mean (and median) percentage of firms that meet or beat the analyst's FFO forecast across the two subperiods, 1995–1999 and 2000–2003. In this manner, we expect to observe a downward shift in the overall tendency of managers to meet or exceed performance targets. Following prior earnings management research that examines the distribution of firms just above and below a particular benchmark (Burgstahler and Dichev 1997; Degeorge et al. 1999), we also investigate the percentage of firms that meet and fail to meet expectations by a small amount. We examine the subset of firms with actual FFO within \$0.05 of the consensus forecast, in an effort to capture the more marginal firms relative to their performance target. If FFO manipulation decreased, we expect to find fewer (more) firms that just meet (fail to meet) expectations.

Figure 1 presents the distribution of unscaled FFO forecast errors over the 1995–1999 and 2000–2003 subperiods. Forecast errors were rounded down to the nearest penny such that \$0.00 includes forecast errors $\geq$ \$0.00 but $<$ \$0.01. (Forecast errors were also truncated at $\pm$ \$0.20 for the purpose of this figure.) We observe a noticeable shift in FFO forecast errors between the two periods, particularly within a narrower range around \$0.00 where we expect FFO manipulation may be most prevalent with respect to meeting/beating the analyst's forecast. A \$0.00 forecast error is the modal value in both subperiods; however, the frequency declines from about 21 percent in 1995–1999 to slightly more than 16 percent in 2000–2003. The percentage of firms with \$0.01 FFO forecast errors drops from about 10 percent (1995–1999) to 8 percent (2000–2003), and we observe similar decreases in firms reporting positive surprises of \$0.02 and \$0.03. We do not find much change in the percentage of firms with $-\$0.01$ FFO forecast errors. However, we see a greater frequency of firms with FFO forecast errors of $-\$0.02$, $-\$0.03$ and $-\$0.04$. In general, the observed shift in FFO forecast errors provides preliminary evidence consistent with a decline in FFO manipulation to meet or beat the analyst's forecast in 2000–2003 relative to 1995–1999.

²⁵ Vincent's (1999) FFO estimation model is GAAP net income plus depreciation and amortization (unless identified as unrelated to real estate), minus gains (losses) related to real estate sales, plus extraordinary items, and minus reported straight-line rents revenues. Since adjusting for straight-line rents are not prescribed by NAREIT, we also estimate the model without that adjustment and find that the error rates are slightly smaller. Estimation errors are defined as reported amounts less estimated amounts.

²⁶ With respect to the overall accuracy of the estimates, we find a mean (median) absolute percentage error of 13.4 percent (10.5 percent) in 1998; Vincent (1999) does not report an unsigned error rate.

FIGURE 1
Percentage of Firm-Year Observations (% of Firms) in the Sample with Unscaled FFO
Forecast Errors in Increments of \$0.01 for the 1995–1999 and 2000–2003 Periods



FFO forecast errors are per share, defined as actual FFO minus the consensus forecast of FFO. These amounts were rounded down to the nearest penny such that \$0.00 includes forecast errors $\geq$ \$0.00 but $<$ \$0.01. Forecast errors were truncated at $-\$0.20$ and $\$0.20$ for the purpose of this graph.

Table 3 provides results of univariate tests of H1. In Panel A, we report descriptive evidence on the percentage of firms meeting or beating the analyst's FFO forecast, failing to meet or beat the analyst's FFO forecast by a small margin (small negative surprises), and meeting or beating the analyst's FFO forecast by a small margin (small positive surprises) for each year of 1995–2003. Panel B presents mean frequencies by subperiod (1995–1999 and 2000–2003). Using a binary variable to classify firms under each of the three conditions, we report both a t-statistic and a Wilcoxon z-statistic to test for differences across the subperiods. Results are consistent with a significant (one-tailed p-value < 0.01) decline in the frequency of meeting or beating the analyst's FFO forecast, as well as the frequency of small positive surprises [$\$0.00, \$0.05$] after 1999. Further, we find a significant (one-tailed p-value of 0.03) increase in the frequency of firms failing to meet or beat the analyst's FFO forecast by a small margin [$-\$0.05, \$0.00$].

TABLE 3
Frequency of Meeting or Beating Analyst's Consensus Forecast

Year	REIT Sample			Non-REIT Non-GAAP Sample			Non-REIT GAAP Sample		
	Meet/ Beat	Small Neg Surp	Small Pos Surp	Meet/ Beat	Small Neg Surp	Small Pos Surp	Meet/ Beat	Small Neg Surp	Small Pos Surp
Panel A: Frequency by Year									
1995	0.65	0.29	0.57	0.63	0.16	0.34	0.68	0.19	0.56
1996	0.58	0.29	0.47	0.64	0.15	0.36	0.72	0.18	0.56
1997	0.55	0.34	0.44	0.65	0.15	0.37	0.71	0.17	0.57
1998	0.53	0.19	0.40	0.67	0.11	0.38	0.72	0.15	0.57
1999	0.60	0.20	0.50	0.68	0.13	0.38	0.75	0.14	0.55
2000	0.55	0.25	0.43	0.63	0.14	0.33	0.74	0.14	0.56
2001	0.54	0.31	0.47	0.71	0.14	0.47	0.72	0.12	0.57
2002	0.43	0.36	0.32	0.69	0.16	0.47	0.71	0.15	0.53
2003	0.46	0.32	0.30	0.69	0.15	0.46	0.73	0.15	0.51
Mean	0.54	0.28	0.43	0.66	0.14	0.39	0.72	0.16	0.55
Panel B: Mean Frequency between 1995–1999 and 2000–2003									
1995–99	0.58	0.26	0.47	0.65	0.14	0.36	0.71	0.17	0.56
2000–03	0.50	0.31	0.38	0.68	0.14	0.42	0.72	0.14	0.54
1995–1999 versus 2000–2003:									
t-test									
p-value:	<0.01	0.03	<0.01	<0.01	0.17	<0.01	0.18	<0.01	0.07
Wilcoxon									
z-test									
p-value:	<0.01	0.03	<0.01	<0.01	0.17	<0.01	0.18	<0.01	0.07

Panel A reports frequencies of meeting or beating the analyst's consensus forecast of FFO (REIT sample of 1,205 firm-year observations) or EPS (Non-REIT Non-GAAP and Non-REIT GAAP samples of 12,630 and 5,957 firm-year observations, respectively) for 1995–2003. Consensus forecasts = the average of each individual analyst's last forecast prior to the announcement date. The Non-REIT Non-GAAP sample firms are those firms where there is non-zero difference between I/B/E/S actual earnings and income before extraordinary items per share (annual Compustat data item 57 or 58, depending on the reported basis of the I/B/E/S actual earnings). The Non-REIT GAAP sample firms include those firms where I/B/E/S actual earnings is equal to income before extraordinary items per share from annual Compustat. Meet/Beat = the frequency that firms' FFO or EPS forecast errors (actual – consensus forecast) are nonnegative; Small Neg Surp = the frequency that firms' FFO or EPS forecast errors are in the range [–\$0.05, \$0.00]; and Small Pos Surp = the frequency that firms' FFO or EPS forecast errors are in the range [\$0.00, \$0.05].

In Panel B, t-test (Wilcoxon z-test) is the one-tailed p-value based on the t-statistic (Wilcoxon signed rank statistic) for a test of differences in the mean (median) frequencies of the 1995–1999 and 2000–2003 periods.

A potential alternative explanation for finding a decrease in meeting or beating expectations is SEC intervention. The SEC's pro forma cautionary advice, issued in December 2001, and Regulation G, effective March 28, 2003, could also play a role in our 2000–2003 test period. We also acknowledge the possibility that other broad-based economic shifts in this time frame could be a factor. To distinguish NAREIT influence from SEC influence and general economic changes, we expand our analysis by identifying two control samples from the non-REIT firms tracked on I/B/E/S. One control sample includes those firms that disclose non-GAAP earnings, characterized by actual EPS on I/B/E/S that differs

from Compustat earnings before extraordinary items (annual data item 57 or 58, depending on whether I/B/E/S actual earnings are reported on a primary or diluted basis). The other control sample includes those firms that disclose GAAP earnings, characterized by actual EPS on I/B/E/S that equals Compustat EPS.²⁷ We repeat the analyses used to test H1 for these two subsamples to ensure that our results are unique to REIT firms. The non-GAAP reporting control sample is intended to capture the influence of SEC intervention on all non-GAAP disclosures during this period; the GAAP earnings subsample should capture any economy-wide shifts in the period that affect unexpected earnings.

Results for the samples of non-REIT firms reporting non-GAAP earnings and those reporting GAAP earnings are also reported in Table 3. Although we make no explicit predictions for these firms, we evaluate the data relative to our FFO predictions. Importantly, neither subsample exhibits the same patterns of change observed for REITs.

The tests described above do not control for other factors that could explain the frequency of meeting or beating expectations. If these factors coincidentally changed around the year 2000, then we might incorrectly assume that industry reporting guidance accounts for any observed shifts in FFO forecast errors. Accordingly, our second test explicitly controls for other variables identified in prior literature (e.g., Matsumoto 2002; Heflin and Hsu 2007) that could affect the frequency of meeting or beating analysts' forecasts. Although these factors have been identified in large cross-sectional analyses and may or may not be relevant within the REIT industry, we include them here to be conservative and estimate the following logistic regression model:

$$\begin{aligned} Pr(\text{Meet or Beat})_t = & \alpha_0 + \alpha_1 * D2000 + \alpha_2 * FFO-P_t + \alpha_3 * MRET_t \\ & + \alpha_4 * FCF_t + \alpha_5 * LSIZE_t + \alpha_6 * BTM_t \\ & + \alpha_7 * GROW_t + \alpha_8 * LEV_t + \alpha_9 * AGE_t + \varepsilon_t \end{aligned} \quad (1)$$

The dependent variable in this model equals 1 if actual FFO meets or beats the analysts' consensus FFO forecast, and 0 otherwise. *D2000* equals 1 for observations in the 2000–2003 period, and 0 otherwise. We expect the coefficient on *D2000* to be negative, indicting a lower frequency of meeting or beating analysts' forecasts in the more recent period that cannot be attributed to other incentives or firm characteristics. We conduct this test for the overall sample of FFO forecast errors and the subset with forecast errors between \$–0.05 and \$0.05.²⁸

Prior research suggests that current period operating performance is positively related to a firm's inherent ability to meet or beat expectations (Heflin and Hsu 2007), so we include several contemporaneous performance measures as controls.²⁹ We calculate price-deflated FFO (*FFO-P*) using prior fiscal year-end price, annual buy-and-hold market-adjusted returns (*MRET*) using the value-weighted market portfolio, and free cash flow

²⁷ Similar to prior research using analyst data services as a source for non-GAAP earnings, we recognize that the actual EPS numbers from I/B/E/S are only a proxy for the numbers actually disclosed by management. Of the 18,587 non-REIT firm-year observations available on I/B/E/S from 1995–2003 (excluding regulated firms), approximately 68 percent of the annual earnings amounts reflect a non-GAAP number. This proportion is comparable to the percentage of firms providing annual non-GAAP earnings in press releases reported in Entwistle et al. (2006).

²⁸ We performed the same analysis using forecast errors falling between $\pm$ \$0.03, \$0.06, and \$0.10. Qualitatively similar results are obtained using these alternatives.

²⁹ All variables are estimated as of the end of fiscal year *t* unless otherwise indicated. Annual returns (firm and market) are for the 12-month period beginning three months after the end of the prior fiscal year and are estimated from monthly CRSP returns.

(*FCF*) defined as operating cash flows minus investing cash flows and dividends. Prior research also suggests less analyst optimism for larger firms (Brown 1997; Das et al. 1998), so we include *LSIZE*, the natural log of market equity. Matsumoto (2002) and Doyle and Soliman (2005) suggest that high-growth firms have greater incentives to meet or beat the analyst's forecast because growth firms suffer a stronger market reaction to negative earnings surprises (Skinner and Sloan 2002). We include the book-equity-to-market-equity ratio (*BTM*) and annual sales growth (*GROW*), measured by dividing current year sales by prior year sales and subtracting 1, to control for growth. We also include a leverage measure, long-term debt to total assets (*LEV*), as a control for any incentives to manage FFO that may be driven by debt constraints. *AGE*, measured as the number of months of trading on a stock exchange, is included to control for the possibility that younger firms with external financing needs manage FFO to impress the capital markets (Fields et al. 1998).

Table 4 presents descriptive data for control variables used in our multivariate logistic regressions of the probability of meeting or beating the analyst's FFO forecast for our REIT sample (Panel A) or EPS forecast for our non-REIT samples (Panels B and C). Additional data restrictions for these tests reduce our REIT sample to 986 observations. Given our focus on reporting changes over time, we report descriptive data by subperiod. Means for some variables appear to have changed between subperiods. In particular, a decrease in growth (*GROW*) and in free cash flows (*FCF*) in our REIT sample could suggest less incentive or ability to meet or beat analyst's forecast in the later period. Alternatively, greater stock returns observed for REITs in the later period, implying improved performance, would likely be associated with a greater potential for meeting or beating expectations in the later years.³⁰

In addition to including other factors that could explain the frequency of meeting or beating expectations, we also estimate Equation (1) after partitioning our 2000–2003 period into two subperiods: *D00-01* and *D02-03*, which are equal to 1 in the years 2000–2001 and 2002–2003, respectively. This additional procedure is used to further control for the SEC's non-GAAP cautionary and regulatory influence, which should be concentrated in the 2002–2003 period, while any NAREIT influence should be evident earlier.

Results, reported in Table 5, for FFO meeting or beating expectations (Panel A) and for meeting or beating expectations by a small margin (Panel B) support the univariate tests in Table 3. In particular, the coefficient on *D2000* is significantly negative (–1.18 in Panel A and –1.25 in Panel B), indicating a downward shift in the probability of meeting or beating the analyst's FFO forecast for 2000–2003 (relative to 1995–1999). Of the control variables, *FFO-P*, *MRET*, *FCF*, *LSIZE*, and *BTM* (Panel A only) are significant in the predicted direction. Importantly, we continue to find a downward shift in the frequency of

³⁰ The stronger stock performance despite slower average growth in the 2000s is consistent with a broader acceptance of REITs among investors, as well as a general trend in the market shifting away from high-growth and technology stocks in favor of dividend paying stocks around this time (Kahn 2002). As mentioned earlier, the REIT industry experienced lackluster performance in 1998–1999. During these years, returns averaged –9 percent, while returns for 1995–1997 averaged 23.9 percent, comparable to the mean returns in 2000–2003. Excluding the years 1998–1999, we find a positive correlation between sales growth and returns of about 11 percent (Spearman) in our REIT sample, similar to a 12 percent correlation in our non-REIT sample for 1995–2003. We also note that among non-REIT firms, our GAAP sample earned greater returns than our non-GAAP sample in both periods, which we believe reflects better overall firm performance. Consistent with Heflin and Hsu (2007), we find that firms reporting non-GAAP earnings had a greater frequency of losses in each sample year.

TABLE 4
Descriptive Statistics for Control Variables Related to Meeting or Beating Expectations

Panel A: REIT Sample

	Total (n = 986)		1995–1999 (n = 529)		2000–2003 (n = 457)	
	Mean	Median	Mean	Median	Mean	Median
<i>FFO-P</i>	0.09	0.10	0.10	0.10	0.09	0.11
<i>RET</i>	0.16	0.16	0.10	0.10	0.24	0.23
<i>MRET</i>	0.05	–0.00	–0.14	–0.16	0.26	0.25
<i>FCF</i>	0.14	0.12	0.20	0.19	0.08	0.08
<i>SIZE</i> (\$million)	960.6	558.0	706.9	465.5	1,254.9	749.9
<i>BTM</i>	0.76	0.67	0.73	0.68	0.79	0.66
<i>GROW</i>	0.32	0.16	0.49	0.30	0.12	0.06
<i>LEV</i>	0.45	0.45	0.44	0.43	0.47	0.47
<i>AGE</i>	105.3	77.0	83.0	52.0	131.2	102.0

Panel B: Non-REIT Non-GAAP Sample

	Total (n = 12,630)		1995–1999 (n = 7,743)		2000–2003 (n = 4,887)	
	Mean	Median	Mean	Median	Mean	Median
<i>EPS-P</i>	0.30	0.01	0.49	0.02	–0.00	0.00
<i>RET</i>	0.21	0.03	0.22	0.06	0.18	–0.03
<i>MRET</i>	0.07	–0.09	–0.02	–0.15	0.21	0.02
<i>FCF</i>	0.15	0.16	0.16	0.17	0.13	0.15
<i>SIZE</i> (\$million)	3,786.2	473.3	2,871.8	366.2	5,234.9	696.1
<i>BTM</i>	0.52	0.41	0.48	0.40	0.59	0.44
<i>GROW</i>	0.54	0.13	0.64	0.15	0.38	0.10
<i>LEV</i>	0.13	0.12	0.14	0.12	0.13	0.12
<i>AGE</i>	163.7	84.0	154.5	73.0	178.3	98.0

Panel C: Non-REIT GAAP Sample

	Total (n = 5,957)		1995–1999 (n = 3,811)		2000–2003 (n = 2,146)	
	Mean	Median	Mean	Median	Mean	Median
<i>EPS-P</i>	0.41	0.04	0.62	0.05	0.05	0.02
<i>RET</i>	0.36	0.15	0.35	0.17	0.38	0.12
<i>MRET</i>	0.19	0.01	0.08	–0.06	0.39	0.17
<i>FCF</i>	0.17	0.19	0.18	0.19	0.16	0.18
<i>SIZE</i> (\$million)	3,024.7	388.3	2,543.3	312.4	3,879.6	542.4
<i>BTM</i>	0.47	0.38	0.45	0.37	0.50	0.38
<i>GROW</i>	0.44	0.16	0.51	0.17	0.31	0.12
<i>LEV</i>	0.12	0.09	0.12	0.09	0.12	0.09
<i>AGE</i>	156.8	99.0	137.1	72.0	169.5	111.0

This table reports descriptive statistics for control variables used in testing whether the frequency of meeting or beating the analyst's consensus FFO or EPS forecast declined (Equation (1)). The Non-REIT Non-GAAP sample (Panel B) firms are those firms for which there is non-zero difference between I/B/E/S actual earnings and income before extraordinary items per share (annual Compustat data item 57 or 58, depending on the reported basis of the I/B/E/S actual earnings). The Non-REIT GAAP (Panel C) sample firms include those firms for

(continued on next page)

TABLE 4 (continued)

which I/B/E/S actual earnings is equal to income before extraordinary items per share from annual Compustat. Consensus forecasts = the average of each individual analyst's last forecast prior to the announcement date.

FFO-P (EPS-P) = FFO (EPS) scaled by stock price as of the end of the prior fiscal year;

RET (MRET) = contemporaneous annual (market-adjusted) buy-and-hold stock returns (using the value-weighted market portfolio) beginning three months after the end of the prior fiscal year;

FCF = free cash flow measured as (operating cash flows – investing cash flows – dividends)/total assets;

SIZE = firm size measured as market value of equity;

BTM = the ratio of book value of equity over market value of equity;

GROW = sales growth measured as (current year's sales divided by prior year's sales) minus 1;

LEV = leverage measured as the ratio of long-term debt to total assets; and

AGE = the number of months the firm's stock has been trading on a stock exchange.

All variables are measured at fiscal year-end unless otherwise noted. *n* = the number of firm-year observations.

FFO meeting or beating expectations after controlling for these other factors.^{31,32} We also find that the decrease in meeting or beating expectations begins in 2000–2001 (Panels A and B), prior to SEC intervention, and continues in 2002–2003, relative to 1995–1999.³³

We also report similar logistic regression results for the non-REIT samples reporting non-GAAP and GAAP earnings in Panels A and B of Table 5. We find no evidence of a decrease in meeting or beating behavior for either non-REIT sample. In fact, the non-REIT GAAP subsample reflects a significant increase in small positive surprises for the years 2000–2001 (coefficient of 0.25, in Panel B, is significantly positive in a two-tailed test). Thus, the observed downward shift in meeting or beating FFO forecasts observed for the REIT industry appears to be distinct from any broader trends in EPS or non-GAAP earnings of other industries.³⁴

Additionally, we consider the possibility that SEC Regulation FD, which limited private communication between management and financial analysts, accounts for the decreased frequency of meeting/beating expectations. Regulation FD, effective October 2000, may

³¹ If some firms have observations only in 1995–1999 or 2000–2003, then changing sample composition may account for the difference. Accordingly, we also conduct similar analysis after excluding the few firms that do not have observations in both time periods. Results are qualitatively similar to those presented.

³² Following Burgstahler and Dichev (1997) and Degeorge et al. (1999) we also consider prior year actuals as an alternative target that firms would want to meet or beat. If opportunistic reporting of FFO diminished in 2000–2003, then we also expect the frequency of FFO increases to decline relative to 1995–1999. Consistent with our analyst forecast results, we find (untabulated) that the frequency of meeting/beating last year's FFO declined from 84 percent in 1995–1999 to 62 percent in 2000–2003 (difference significant at <0.01 level). Results for firms with small earnings changes (within one standard deviation of the mean change) are similar. Further, we find the decline to be significant after controlling for the economic factors used in Table 5, consistent with a decrease in managerial discretion.

³³ In untabulated analysis, we also estimate Equation (1) after excluding 2003 observations to rule out the effects of SEC Regulation G. Setting *D2000* equal to 1 for the 2000–2002 period, and 0 otherwise, we still find a significant decrease beginning in 2000, similar to the results presented in Panels A and B.

³⁴ Our failure to find a decrease in meeting/beating analysts' forecasts for 2002–2003 in the non-REIT samples contrasts with Heflin and Hsu (2007), who find a decrease after implementation of SEC Regulation G. We note several important differences between our study and theirs. Whereas we aggregate annual earnings data for 2000–2003 (and 2002–2003) and test for changes relative to 1995–1999, Heflin and Hsu (2007) use quarterly earnings data from 2000–2004 to test for an incremental shift in the first quarter of 2003. In unreported analysis, we modify our Logit model to specifically test for a decline in 2003 relative to 2000–2002 and observe a negative but insignificant coefficient. Their use of quarterly earnings data results in a larger sample than our annual sample, which increases the statistical power of their tests. Further, Heflin and Hsu (2007) find significantly less meeting/beating of analysts' forecasts in the fourth quarter (relative to the first three quarters), and an earlier version of their paper suggests that most of the decrease in 2003 can be attributed to the first three quarters (as opposed to the fourth quarter). We expect that our annual forecast errors (measured late in the year) primarily reflect the fourth quarter surprise.

TABLE 5
Results of Logistic Regression of the Probability of Meeting or Beating the Analyst's
Consensus Forecast on Firm Characteristics

Variables	Pred. Sign	REIT Sample		Non-REIT Non-GAAP Sample		Non-REIT GAAP Sample	
		Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Panel A: Meeting/Beating Analysts' Consensus Forecast for 1995–1999 versus 2000–2003							
Intercept		−0.80 0.19	−0.80 0.20	0.05 0.54	0.05 0.51	0.46 <0.01	0.45 <0.01
<i>D2000</i>	−	−1.18 <0.01		0.02 0.67		0.01 0.56	
<i>D00-01</i>	−		−1.13 <0.01		−0.01 0.42		0.06 0.75
<i>D02-03</i>	−		−1.21 <0.01		0.06 0.85		−0.03 0.34
<i>FFO-P</i>	+	13.00 <0.01	12.83 <0.01				
<i>EPS-P</i>	+			0.00 0.40	0.00 0.40	0.00 0.12	0.00 0.12
<i>MRET</i>	+	1.38 <0.01	1.35 <0.01	0.16 <0.01	0.16 <0.01	0.14 <0.01	0.14 <0.01
<i>FCF</i>	+	1.00 0.05	1.01 0.05	0.27 <0.01	0.27 <0.01	1.05 <0.01	1.04 <0.01
<i>LSIZE</i>	+	0.19 <0.01	0.19 <0.01	0.12 <0.01	0.11 <0.01	0.10 <0.01	0.10 <0.01
<i>BTM</i>	−	−1.27 <0.01	−1.26 <0.01	−0.02 0.15	−0.02 0.16	−0.23 <0.01	−0.24 <0.01
<i>GROW</i>	+	0.01 0.47	0.01 0.47	−0.01 0.95	−0.01 0.96	−0.01 0.89	−0.01 0.89
<i>LEV</i>	?	−0.41 0.43	−0.40 0.43	−0.61 <0.01	−0.61 <0.01	−0.96 <0.01	−0.96 <0.01
<i>AGE</i>	−	−0.00 0.20	−0.00 0.21	−0.00 0.01	−0.00 0.01	−0.00 <0.01	−0.00 <0.01
Log likelihood		−626.09	−626.02	−7952.1	−7951.6	−3432.9	−3434.5
Chi-square		93.61	93.73	237.11	238.23	230.64	231.44
p-value		<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
n		986	986	12,630	12,630	5,957	5,957
Panel B: Meeting/Beating Analysts' Consensus Forecast by a Small Margin for 1995–1999 versus 2000–2003							
Intercept		−0.89 0.31	−0.82 0.35	0.72 <0.01	0.72 <0.01	1.07 <0.01	1.06 <0.01
<i>D2000</i>	−	−1.25 <0.01		0.06 0.85		0.13 0.94	
<i>D00-01</i>	−		−1.06 <0.01		0.04 0.71		0.25 0.99
<i>D02-03</i>	−		−1.33 <0.01		0.09 0.88		0.03 0.61
<i>FFO-P</i>	+	8.29 <0.01	7.54 0.01				

(continued on next page)

TABLE 5 (continued)

Variables	Pred. Sign	REIT Sample		Non-REIT Non-GAAP Sample		Non-REIT GAAP Sample	
		Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Panel B: Meeting/Beating Analysts' Consensus Forecast by a Small Margin for 1995–1999 versus 2000–2003							
EPS-P	+			-0.00 0.64	-0.00 0.64	-0.00 0.75	-0.00 0.75
MRET	+	1.59 <0.01	1.47 <0.01	0.12 <0.01	0.12 <0.01	0.10 0.02	0.10 0.03
FCF	+	1.44 0.03	1.46 0.03	0.16 0.10	0.16 0.10	0.91 <0.01	0.91 <0.01
LSIZE	+	0.16 0.03	0.16 0.03	0.05 <0.01	0.05 <0.01	0.04 0.07	0.05 0.05
BTM	–	-0.24 0.27	-0.24 0.27	0.01 0.62	0.01 0.62	-0.29 <0.01	-0.29 <0.01
GROW	+	-0.25 0.94	-0.26 0.94	-0.01 0.77	-0.01 0.76	-0.00 0.88	-0.01 0.88
LEV	?	0.03 0.96	0.03 0.96	-0.38 0.10	-0.38 0.10	-0.55 0.09	-0.58 0.08
AGE	–	-0.00 0.26	-0.00 0.26	-0.00 0.12	-0.00 0.12	-0.00 <0.01	-0.00 <0.01
Log likelihood		-468.94	-468.38	-3852.29	-3852.13	-2209.28	-2207.82
Chi-square		37.99	39.13	31.07	31.40	90.81	93.74
p-value		<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
n		735	735	6,665	6,665	4,246	4,246

Panel A reports coefficients and p-values from logistic estimation of the probability of meeting or beating the analyst's consensus forecast. The dependent variable in Panel A = 1 if actual FFO (REIT Sample) or EPS (Non-REIT Non-GAAP and Non-REIT GAAP Samples) meets or beats the analyst's consensus forecast, and 0 otherwise. The Non-REIT Non-GAAP sample firms are those firms where there is non-zero difference between I/B/E/S actual earnings and income before extraordinary items per share (annual Compustat data item 57 or 58, depending on the reported basis of the I/B/E/S actual earnings). The Non-REIT GAAP sample firms include those firms for which I/B/E/S actual earnings is equal to income before extraordinary items per share from annual Compustat. Consensus forecasts = the average of each individual analyst's last forecast prior to the announcement date.

Panel B reports coefficients and p-values from logistic estimation of the probability of meeting or beating the analyst's consensus forecast by a small margin. The dependent variable in Panel B = 1 if actual FFO (REIT Sample) or EPS (Non-REIT Non-GAAP and Non-REIT GAAP Samples) meets or beats the analyst's consensus forecast by 5 cents or less, and 0 if actual FFO or EPS misses the analyst's consensus forecast by 5 cents or less.

D2000 = 1 for the years 2000–2003, and 0 otherwise;

D00-01 (D02-03) = 1 for the years 2000–2001 (2002–2003), and 0 otherwise;

FFO-P (EPS-P) = FFO (EPS) scaled by stock price as of the end of the prior fiscal year;

MRET = annual buy-and-hold market-adjusted stock returns using the value-weighted market portfolio beginning three months after the end of the prior fiscal year;

FCF = free cash flow measured as (operating cash flows – investing cash flows – dividends)/total assets;

LSIZE = firm size measured as the natural logarithm of market value of equity;

BTM = the ratio of book value of equity over market value of equity;

GROW = sales growth measured as (current year's sales divided by prior year's sales) minus 1;

LEV = leverage measured as the ratio of long-term debt to total assets; and

AGE = the number of months the firm's stock has been trading on a stock exchange.

All variables are measured at fiscal year-end unless otherwise noted. n = the number of firm-year observations. The p-values reflect a one-tailed test when the sign is predicted; two-tailed test otherwise.

P 31200

have curtailed managers' ability to manipulate analyst expectations and thereby make it more difficult to produce positive earnings surprises in recent years. We evaluate this possibility by examining the likelihood of meeting/beating expectations while controlling for changing expectations over the fiscal year. Figure 2 charts the percentage of non-negative FFO surprises using different monthly consensus forecasts (beginning eight months before the FFO announcement) in both the pre- and post-Reg FD periods. For the pre-Reg FD period (1995–1999), we find that 55 percent of firms report annual FFO that meets or beats the consensus forecast eight months prior to the announcement, while 58 percent meet or beat the consensus forecast in the month of the announcement. This 3 percent increase in positive surprises is consistent with some downward expectations management over the period. However, in the post-Reg FD period (2000–2003), we find changing expectations account for an 11 percent increase in meeting/beating forecasts (39 percent using the consensus forecast eight months prior to the announcement and 50 percent using the consensus forecast in the announcement month). These results suggest that downward expectations management directed at meeting/beating analysts' forecasts was *more* evident in the post-Reg FD period. Despite this strategy, we still find less frequent meeting or beating of expectations in 2000–2003.³⁵

Additional Evidence of Increased Uniformity and Decreased Discretion

For a random sample of 51 REITs that disclose details of their FFO computation, we also examine the specific adjustments that reconcile GAAP net income to FFO. Limiting our analysis to two three-year windows, 1997–1999 and 2000–2002, we count the number of adjustments to net income that do not conform to NAREIT's 1999 definition. These "exceptions" generally involve nonrecurring income items that management eliminates in computing FFO.³⁶ If REITs altered their behavior to conform to the 1999 Policy Bulletin, then we expect a decline in the frequency of nonrecurring adjustments (i.e., fewer exclusions of nonrecurring items) in 2000–2002.

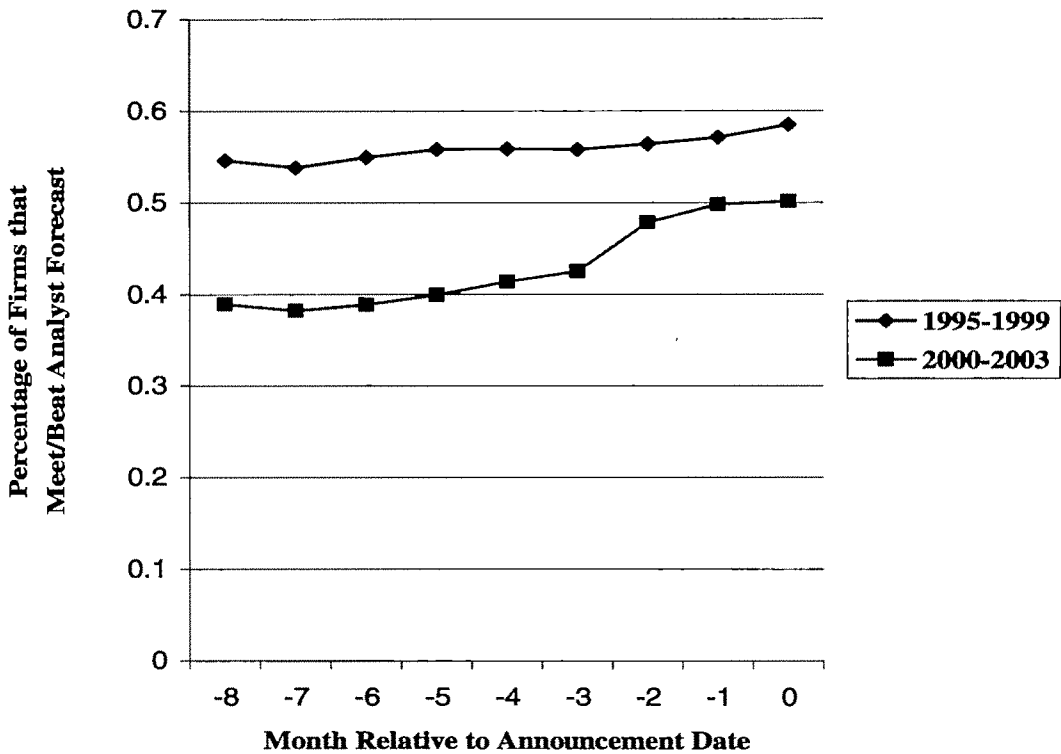
Results (untabulated) are consistent with our expectations. For 1997–1999, we identify 161 individual exceptions, an average of three per firm. Of this total, significantly more adjustments are FFO-increasing (132, or 82 percent) than FFO-decreasing (29, or 18 percent).³⁷ We also confirm that the aggregate dollar effect of these exception adjustments (per firm-year) is more frequently a net increase (82 percent) than net decrease (18 percent) in reported FFO. For these same firms in 2000–2002, we identify only 77 individual exceptions (an average of 1.5 for firm), of which significantly more are FFO-increasing (61, or 79 percent) than decreasing (16, or 21 percent). Compared to 1997–1999, both FFO-increasing and decreasing exceptions decline (54 percent and 45 percent, respectively) in 2000–2002, and the decline in FFO-increasing items is significantly greater than for FFO-decreasing items. Comparing the net dollar effects of the adjustments across periods, we

³⁵ Using First Call management forecast data, we also examine guidance provided by REIT managers in the pre- and post-Reg FD periods. Corroborating the evidence in Figure 2, we find (untabulated) a greater frequency of downward guidance in the post-Reg FD period (64 percent) than in the pre-Reg FD period (45 percent). Collectively, these findings suggest to us that REITs may have increased their efforts to manipulate FFO expectations beginning in 2000, because they had less flexibility to manipulate reported FFO. It does not appear that reduced communication due to Regulation FD can explain the overall decrease in meeting/beating analysts' FFO forecasts.

³⁶ Under NAREIT's 1995 definition, nonrecurring gains/losses were excluded from FFO, but under the 1999 definition, they should be included in FFO. Specific examples include those discussed in footnote 12 as well as amortization of deferred financing costs (non-extraordinary) and costs related to litigation settlement, environmental remediation, lease termination, post-retirement benefits, and acquisition financing.

³⁷ Test results for analysis of exceptions are based on Mann-Whitney tests. Significance indicates p-values ≤ 0.05 .

FIGURE 2
Time-Series Movement in Analysts' FFO Forecasts Relative to Announcement Date



Percent of firms that meet/beat analyst forecast is the frequency that firms' FFO forecast errors (actual - consensus forecast) are nonnegative. The month relative to announcement date refers to how many months prior to the annual FFO announcement the consensus forecast is measured for the purpose of calculating a forecast error. Consensus forecast = the average of each individual analyst's last forecast issued in the respective month.

similarly find a greater decline in the frequency of net dollar increases than in net dollar decreases (44 percent versus 21 percent). In summary, relative to 1997-1999, both positive and negative FFO exception adjustments decline in 2000-2002, however, we find that the reduction is significantly more pronounced for the FFO increasing items. It also appears that firms reported fewer nonrecurring adjustments after 1999. This result is important because it indicates that many REITs followed NAREIT's guidance in the 1999 National Policy Bulletin, which in turn curbed managerial discretion concerning the determination of nonrecurring items and limited the ability to inflate FFO.

Testing for Increase in Information Content of FFO to Investors

Our second hypothesis considers whether investors respond to these initiatives and find FFO to be more informative for firm valuation. We test this by assessing whether the market response to FFO changed over the 1995-1999 and the 2000-2003 subperiods, as estimated by the following ordinary least squares regression:

$$CAR_t = \beta_0 + \beta_1 * D2000 + \beta_2 * UFFO_t + \beta_3 * UFFO_t * D2000 + \varepsilon_t \quad (2)$$

CAR is the cumulative abnormal stock return over the three-day window surrounding the FFO announcement date (−1, 1). We use announcement date returns to assess the unique information content of FFO. Abnormal returns are based on a market model estimated using the CRSP value-weighted return over days −220 to −20 relative to the announcement date. *UFFO* is the FFO forecast error scaled by beginning-of-year stock price, and *D2000* is as defined earlier. We expect a positive coefficient on the interaction term, *UFFO* * *D2000*, consistent with an upward shift in the information content of FFO in the 2000–2003 period.

Results presented in Table 6, Panel A and Model (1) support the information content of FFO to investors by finding a significant (one-tailed *p*-value <0.01), positive relation between unexpected FFO (*UFFO*) and announcement period CAR for the entire sample period. In Model (2), the coefficient on *UFFO* * *D2000* of 1.39 is significant (one-tailed *p*-value of 0.02), consistent with incrementally greater informativeness of FFO after 1999, supporting H2.³⁸ Furthermore, this result is observed in both the 2000–2001 and 2002–2003 subperiods, consistent with the increased informativeness of FFO preceding SEC intervention. Model (3) documents a significant positive coefficient (0.36) during 1995–1999, and a significant positive shift of 0.74 (2.24) in 2000–2001 (2002–2003).

In comparison, results from estimating Model (1) with the two non-REIT samples, presented in Panel B, also reflect greater information content in both non-GAAP and GAAP earnings after 1999. However, as reported in Model (2), the increased market response to earnings of these firms is restricted to the 2002–2003 time period. Therefore, the shift in FFO information content among REITs precedes any shift with other earnings measures in other industries and suggests to us that NAREIT's guidance played a unique role in altering investor perceptions. The greater information content observed for non-REIT, non-GAAP firms for 2002–2003 is consistent with Kolev et al. (2008), Zhang and Zheng (2004), and Yi (2007), who find improved non-GAAP disclosures in the post-Reg G period. Our finding greater information content for non-REIT, GAAP firms for 2002–2003 is somewhat puzzling. One possibility is that the increased attention regulators were giving to the quality of earnings-related disclosures during this period of time resulted in a more general real and/or perceived improvement in earnings quality. However, investigating that issue is beyond the scope of the current study.

Our test of H2 implicitly assumes that investor responses to FFO are a function of the perceived reliability of FFO. We attribute the stronger market reaction to FFO in 2000–2003 to investors rationally perceiving greater FFO reliability. Alternatively, we acknowledge that investors may have overestimated the increase in FFO reliability. If investors behaved irrationally by placing greater weight on unexpected FFO, we would expect a future price reversal reflected in a negative correlation between unexpected FFO and future stock returns. In untabulated analysis, we fail to find a significant correlation between year-ahead stock returns and unexpected FFO over the entire sample period or in either subperiod. While these results are consistent with on-average efficient pricing of FFO, before and after NAREIT's initiatives, they do not preclude market mispricing of FFO for some subset of firms in our sample.

³⁸ Finding a significantly larger coefficient on *UFFO* for 2000–2003 also indicates that the rewards (and penalties) for meeting/beating (failing to meet/beat) have not declined for REITs in 2000–2003 relative to 1995–1999. Thus, it is unlikely that our finding support for H1 (i.e., a decrease in meeting/beating expectations) can be explained by a decrease in the incentives to meet market expectations.

TABLE 6
Announcement Period Information Content of Unexpected FFO and EPS

Panel A: Incremental Information Content of *UFFO* for 2000–2003 Relative to 1995–1999 for the REIT Sample

	<u>Intercept</u>	<u>D2000</u>	<u>D00-01</u>	<u>D02-03</u>	<u>UFFO</u>	<u>UFFO*</u> <u>D2000</u>	<u>UFFO*</u> <u>D00-01</u>	<u>UFFO*</u> <u>D02-03</u>	<u>Adj.</u> <u>R²</u>	<u>n</u>
Predicted Sign					+	+	+	+		
(1)	-0.001 (0.14)				0.80 (<0.01)				0.03	981
(2)	-0.001 (0.10)	0.001 (0.28)			0.35 (0.16)	1.39 (0.02)			0.05	981
(3)	-0.002 (0.12)		0.003 (0.21)	0.001 (0.65)	0.36 (0.02)		0.74 (0.03)	2.24 (<0.01)	0.06	981

Panel B: Incremental Information Content of *UEPS* for 2000–2003 Relative to 1995–1999 for the Non-REIT Samples

	<u>Intercept</u>	<u>D2000</u>	<u>D00-01</u>	<u>D02-03</u>	<u>UEPS</u>	<u>UEPS*</u> <u>D2000</u>	<u>UEPS*</u> <u>D00-01</u>	<u>UEPS*</u> <u>D02-03</u>	<u>Adj.</u> <u>R²</u>	<u>n</u>
Predicted Sign					+	+	+	+		
Non-REIT Non-GAAP Sample										
(1)	0.003 (<0.01)	-0.003 (0.15)			0.31 (<0.01)	0.40 (0.03)			0.004	12,308
(2)	0.003 (<0.01)		0.001 (0.74)	-0.008 (<0.01)	0.31 (<0.01)		0.28 (0.12)	2.08 (<0.01)	0.01	12,308
Non-REIT GAAP Sample										
(1)	0.008 (<0.01)	-0.007 (<0.01)			0.72 (<0.01)	0.71 (0.05)			0.01	5,877
(2)	0.008 (<0.01)		-0.006 (0.08)	-0.009 (<0.01)	0.72 (<0.01)		0.39 (0.23)	1.94 (<0.01)	0.02	5,877

This table reports results of regressing three-day cumulative abnormal returns (CAR) centered on the announcement date ($-1,1$) on funds from operations (FFO) or earnings per share (EPS) forecast errors. The Non-REIT Non-GAAP sample firms are those firms where there is non-zero difference between I/B/E/S actual earnings and income before extraordinary items per share (annual Compustat data item 57 or 58, depending on the reported basis of the I/B/E/S actual earnings). The Non-REIT GAAP sample firms include those firms for which I/B/E/S actual earnings is equal to income before extraordinary items per share from annual Compustat. Abnormal returns are computed by using the market model estimated over days -220 to -20 relative to the announcement date.

UFFO = unexpected FFO measured as (actual FFO – consensus FFO)/one-year lagged price;

UEPS = unexpected EPS measured as (actual EPS – consensus EPS)/one-year lagged price. The consensus forecast = the average of each individual analyst's last forecast prior to the announcement date;

D2000 = 1 for the years 2000–2003, and 0 otherwise; and

D00-01 (*D02-03*) = 1 for the years 2000–2001 (2002–2003), and 0 otherwise.

Regressions are estimated using White's (1980) correction for heteroscedasticity. The numbers in parenthesis denote p-values for a one-tailed test when the sign is predicted, and two-tailed test otherwise. *n* = the number of firm-year observations.

Testing FFO Information Content for Firms that Reconcile to EPS

Our third hypothesis considers the transparency of a firm's FFO disclosure. NAREIT encouraged firms to voluntarily reconcile FFO to EPS throughout our sample period. Because most REITs voluntarily adopted the reconciliation prior to the SEC requirement, we can assess whether investors find FFO of firms that reconcile to be more transparent and have more information content. We test this hypothesis by estimating the following model:

$$CAR_t = \beta_0 + \beta_1 * RECON + \beta_2 * UFFO_t + \beta_3 * UFFO_t * RECON + \varepsilon_t \quad (3)$$

RECON is an indicator variable equal to 1 for firms disclosing a reconciliation of FFO with GAAP earnings, and 0 otherwise. All other variables are as previously described. Finding a positive coefficient on the *UFFO* * *RECON* interaction would be consistent with greater transparency that improves the reliability of the non-GAAP measure, giving credence to the SEC reconciliation requirement. Because virtually all REITs report a reconciliation in the later subperiod, which we argue reflects less FFO manipulation, we also estimate Equation (3) with the *UFFO* * *D2000* interaction to control for this effect.

Table 7 provides results from testing H3. In Panel A, using observations from 1995–2003, the coefficient on *UFFO* * *RECON* of 1.29 in Model (1) is significant (one-tailed p-value <0.01), consistent with incrementally greater value relevance of FFO when supplemented by a reconciliation of FFO to earnings. The coefficient on *UFFO* for firms not reporting a reconciliation is not significant (one-tailed p-value of 0.69). This result provides some evidence supporting the SEC's requirement that firms disclosing a non-GAAP performance measure also provide a reconciliation to the most directly comparable GAAP measure.

As discussed earlier, the frequency with which REITs provided a reconciliation increased over time. This coincides with NAREIT's efforts to decrease manipulation in FFO by reducing discretion and promoting a more uniform definition. Therefore, we next attempt to disentangle the benefits of providing a reconciliation from the benefits of more uniformity by including both the *D2000* and *RECON* interactions with *UFFO*. Results, presented in Model (2) of Panel A, suggest that the two effects are unique. We find an incremental increase in the coefficient relating *UFFO* to *CAR* both for the 2000–2003 period (coefficient of 1.30 with p-value of 0.03) and for firms reporting a reconciliation (coefficient of 0.92 with p-value of 0.05). Likewise, in Model (3) where we separately control for the two subperiods, 2000–2001 and 2002–2003, we find significant interactions for both time periods and the reconciliation effect.

Prior studies suggest that a firm's decision to disclose voluntarily may be related to characteristics of the firm's information environment (e.g., earnings information quality), strategic efforts by management (e.g., disappointing GAAP earnings), and/or a demand for information by sophisticated investors (e.g., institutional investors).³⁹ Based on these findings, we consider whether our finding a positive coefficient for the interaction term *UFFO* * *RECON* may reflect some unidentified firm characteristic that is correlated with the reconciliation decision and market returns.

³⁹ For example, Chen et al. (2002) find greater balance sheet disclosure for firms with less informative current earnings and firms with more uncertain future earnings. Lougee and Marquardt (2004) find that firms are more likely to disclose pro forma earnings when GAAP earnings are less informative and when GAAP earnings fail to meet a benchmark. Zhang and Zheng (2004) also present evidence that the quality of voluntary disclosures is greater when the informativeness of GAAP earnings is lower, GAAP earnings fails to meet a benchmark, and the demand for information by sophisticated investors is greater.

TABLE 7
Announcement Period Information Content of Unexpected FFO Conditional on a Reconciliation

Panel A: Incremental Information Content of *UFFO* Conditional on a Reconciliation and 2000–003 for the REIT Sample

Intercept	<i>D2000</i>	<i>D00-01</i>	<i>D02-03</i>	<i>RECON</i>	<i>UFFO</i>	<i>UFFO* D2000</i>	<i>UFFO* D00-01</i>	<i>UFFO* D02-03</i>	<i>UFFO* RECON</i>	Adj. R ²	n
Predicted Sign					+	+	+	+	+		
(1)	-0.004 (0.10)			0.003 (0.24)	-0.38 (0.69)				1.29 (<0.01)	0.04	981
(2)	-0.004 (0.08)	0.001 (0.44)		0.002 (0.32)	-0.46 (0.85)	1.30 (0.03)			0.92 (0.05)	0.05	981
(3)	-0.004 (0.13)		0.002 (0.30)	0.001 (0.82)	-0.43 (0.60)		0.67 (0.04)	2.14 (<0.01)	0.89 (0.05)	0.06	981

Panel B: Incremental Information Content of *UFFO* Conditional on a Reconciliation and Firm Characteristics for 1995–1999 for the REIT Sample

Intercept	<i>RECON</i>	<i>UFFO</i>	<i>UFFO*RECON</i>	<i>UFFO*FOLLOW</i>	<i>UFFO*LSIZE</i>	<i>UFFO*ABSDIFF</i>	Adj. R ²	n
Predicted Sign		+	+					
-0.004 (0.16)	0.002 (0.44)	2.32 (0.04)	1.15 (0.04)	0.30 (<0.01)	-0.89 (<0.01)	0.01 (0.96)	0.06	521

This table reports results of regressing three-day cumulative abnormal returns (CAR) centered on the announcement date (-1,1) on funds from operations (FFO) forecast errors conditional on a reconciliation and firm characteristics. Abnormal returns are computed by using the market model estimated over days -220 to -20 relative to the announcement date.

UFFO = unexpected FFO measured as (actual FFO - consensus FFO)/one-year lagged price. The consensus forecast = the average of each individual analyst's last forecast prior to the announcement date;

D2000 = 1 for the years 2000–2003, and 0 otherwise;

D00-01 (*D02-03*) = 1 for the years 2000–2001 (2002–2003), and 0 otherwise;

RECON = 1 for firm-year observations where a reconciliation of FFO to earnings is provided, and 0 otherwise;

FOLLOW = the number of analysts following the firm;

LSIZE = the natural logarithm of market value of equity; and

ABSDIFF = the absolute difference between earnings per share and FFO.

Regressions are estimated using White's (1980) correction for heteroscedasticity. The numbers in parenthesis denote p-values for a one-tailed test when the sign is predicted, and two-tailed test otherwise. n = the number of firm-year observations.

We note that our setting is somewhat different from prior studies. First, our reconciliation results reported in Table 7, Panel A, are within a sample of firms that nearly all voluntarily disclosed an FFO-EPS reconciliation by 2000. In other words, we are not contrasting a mutually exclusive set of reconciling and non-reconciling firms. Rather, our results explain the variation within these firms, such that we contrast years with a reconciliation versus years with no reconciliation. Thus, we examine whether reconciliation (non-reconciliation) firm-years correspond to firm-years where earnings are less (more) informative, strategic efforts are greater (lower), or investor demand for information is higher (lower). A second important difference with our study is that the disclosure policy change appears to be permanent in that our sample firms continuously provided a reconciliation once they started. As a result, we expect that in our setting it is less likely that managers disclose a reconciliation as a period-specific, strategic choice to compensate for lower earnings quality (e.g., a GAAP earnings loss) or to influence investor perceptions of current performance (e.g., when GAAP earnings fail to meet a benchmark). To be conservative, we still evaluate these possibilities.

We partition our sample into reconciliation and non-reconciliation firm-years. Additionally, we restrict the analysis to 1995–1999 since nearly all REITs provide reconciliations after 1999 and to eliminate any confounding effects of NAREIT's 1999 efforts to increase FFO uniformity. We examine whether variables proxying for earnings informativeness, strategic incentives, and investor demand for information vary between our non-reconciliation and reconciliation subsamples. Based on prior studies, we measure earnings informativeness by estimating the earnings response coefficient (*ERC*) and by the frequency of losses (*LOSS*); we measure strategic efforts to influence investors as the frequency of negative earnings but positive FFO (*REVERSELOSS*); and we measure investor informational demands by analyst following (*FOLLOW*) and percentage of institutional holdings (*PINST*).

As expected, we find no evidence that the reconciliation decision corresponds to either decreases in earnings informativeness or increases in strategic efforts to influence investors. Comparing the reconciliation and non-reconciliation subsamples, we fail to find significant differences in either the *ERC* (p-value of 0.40) or *LOSS* (p-value of 0.27).⁴⁰ We also fail to find a significant difference in *REVERSELOSS* (p-value of 0.38) between the two subsamples. We do find evidence that the reconciliation disclosure policy is correlated with a demand for information by sophisticated investors, as indicated by greater analyst following in the reconciliation subsample (p-value of <0.01), though we do not find a difference in institutional holdings across subsamples (p-value of 0.21).

Finally, we also consider whether the reconciliation decision is correlated with firm size (*LSIZE*), measured as the log of market equity, or the absolute difference between EPS and FFO (*ABSDIFF*).⁴¹ *Ex ante* we expect that managers would be more likely to provide voluntary disclosure when *ABSDIFF* is greater, however we find marginally smaller *ABSDIFF* in the reconciliation subsample (p-value of 0.10). We do find that firm size is greater on average in the reconciliation subsample (p-value of <0.01).

⁴⁰ ERCs are estimated by regressing stock returns on earnings changes. Given an insufficient number of observations for firm-specific models, we estimate ERCs in a pooled cross-sectional model with an interactive indicator variable to distinguish the two subsamples. We do find significant ERCs (p-value of 0.05) in both the non-reconciliation and reconciliation subsamples, but an insignificant difference between groups. We evaluate the other potentially correlated variables by testing differences in means between the reconciliation and non-reconciliation partitions.

⁴¹ Lang and Lundholm (1993) argue that larger firms may be motivated to voluntarily disclose more information because they face greater threat of litigation and disclosure can deter litigation. Also, investors in large capitalization stocks are likely to demand more disclosure due to greater potential gains from trading on that information. Zhang and Zheng (2004) also suggest that voluntary disclosure may be related to *SIZE* and *ABSDIFF*.

Given our findings linking reconciliation policy to *FOLLOW*, *LSIZE*, and *ABSDIFF*, we re-estimate Equation (3) for the 1995–1999 period after including the interaction of these control variables with *UFFO* in the model. Results in Panel B of Table 7 indicate that the FFO surprise is significantly related to announcement period returns ($UFFO = 2.32$), and the reconciliation disclosure significantly enhances that relation ($UFFO * RECON = 1.15$) even after controlling for *FOLLOW*, *LSIZE*, and *ABSDIFF*. Overall, these results support H3 in the years prior to NAREIT's 1999 National Policy Bulletin.

V. CONCLUSION

We find evidence that industry guidance can affect non-GAAP reporting practices. Results indicate a significant decrease in the frequency of meeting or beating analysts' expectations of funds from operations (FFO) in the real estate investment trust (REIT) industry following the trade association's 1999 policy bulletin directed at reducing discretion and increasing uniformity related to FFO. We also observe a shift away from small positive FFO surprises and an increase in small negative FFO surprises. These changes appear to be unique to FFO and the REIT industry. These and other results we report suggest that industry efforts to reduce discretion in FFO also reduced opportunistic reporting. We further find a corresponding increase in the usefulness of FFO to investors, suggesting that greater uniformity enhanced the perceived reliability of FFO for valuation purposes. Furthermore, we trace some of FFO's increased value relevance to the practice of reconciling FFO to GAAP EPS, which makes departures from GAAP more transparent. We therefore conclude that industry and market forces play a critical role in monitoring and influencing such disclosure. Ultimately, these efforts can curb opportunistic reporting and enhance credibility from an investor perspective.

APPENDIX EXAMPLES OF FFO DISCLOSURE

This appendix includes two examples of FFO disclosures reported in the firm's 10-K filing. These firms were specifically chosen because they include a number of reconciling items, but the nature of the adjustments themselves were observed in other REITs.

Case 1

Prime Group Realty Trust, Year Ended December 31, 1999 (in thousands)

Net income allocated to common shareholders	\$42,745
FFO adjustments:	
Real estate depreciation and amortization (1)	30,549
Amortization of costs for leases assumed	899
Straight-line rental revenue adjustments	(3,263)
Joint venture adjustments	654
Gain on sales of real estate	(52,706)
Adjustment associated with Services Company	2,783
Net (gain) loss on Treasury Lock terminations	(615)
Loss on land development options interests	600
Minority interests	30,687
Extraordinary loss	1,082
Funds from operations	<u>\$53,415</u>

(1) Excludes the amortization of deferred financing costs and non-real-estate-related depreciation.

In Case 1, the company begins with net income available to common shareholders, rather than total income (\$54,948). We observe many REITs reporting FFO available to common shareholders. The amounts disclosed for real estate depreciation and amortization, joint venture adjustments, and Service Company adjustment do not appear elsewhere in the financial statements. The amortization of lease costs, straight-line rent adjustment, and treasury lock termination loss appear as adjustments in the statement of cash flows. The loss on land development options, minority interest, and extraordinary loss are itemized in the income statement. Gain on sales of real estate also appears in the income statement, but the amount (\$53,050) differs from above, and total depreciation and amortization (\$33,258) are reported in the income statement. Using available annual report data to estimate FFO with the formula from Vincent (1999) would produce an FFO estimate of \$32,975 (38 percent less than the reported amount).

Case 2

Taubman Centers, Inc.

Year Ended December 31, 1998 (in millions)

Income before extraordinary items and minority interest (1)	70.4
Restructuring charge	10.7
Depreciation and Amortization (2)	57.4
Share of Unconsolidated Joint Ventures' depreciation and amortization (3)	20.7
Other income/expenses, net	0.5
Non-real estate depreciation	(2.3)
Preferred dividends	(16.6)
Funds from Operations	<u>140.8</u>

- (1) Includes gains on peripheral land sales of \$6.0 million for the year ended December 31, 1998.
- (2) Includes \$2.7 million of mall tenant allowance amortization.
- (3) Includes \$1.3 million of mall tenant allowance amortization.

In Case 2, the company begins with net income before extraordinary items and minority interest, rather than total income (\$13.62 million). This starting point occurred often among REITs. The amount for depreciation and amortization agreed with the income statement, although the company's share of joint venture depreciation and amortization and non-real estate depreciation could not be located elsewhere in the financial statements. It is unclear what the adjustment for \$0.5 million other expenses relates to. The note to the reconciliation reveals that the company included gains on land sales; however, companies have the option to exclude gains/losses on undepreciated land along with depreciated property. The \$6 million gains on land sales also appears in the cash flow statement but is not designated as peripheral. Our FFO estimation approach relying on other financial statement information would result in an FFO estimate of \$115.77 million (25 percent less than the reported amount).

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Earnings Surprises that Motivate Analysts to Reduce Average Forecast Error

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ABSTRACT: Large earnings surprises and negative earnings surprises represent more egregious errors in analysts' earnings forecasts. We find evidence consistent with our expectation that egregious forecast errors motivate analysts to work harder to develop or acquire relatively more private information in an effort to avoid future forecasting failures. Specifically, we find that after large or negative earnings surprises there is a greater reduction in the error in individual analysts' forecasts of future earnings, and these individual forecasts are based more heavily on individual analysts' private information. This increased reliance on private information reduces the error in the mean forecast of upcoming earnings (even after controlling for the effect of reduced error in individual forecasts). As reliance on private information increases, more of each individual forecast error is idiosyncratic, and thus averaged out in the computation of the mean forecast.

Keywords: *BKLS; analyst forecasts; forecast error; earnings surprises.*

Data Availability: *All data are available from public sources.*

I. INTRODUCTION

Large earnings surprises and negative earnings surprises represent egregious forecasting failures for analysts. Large earnings surprises hurt analysts' reputations and even threaten their job security (see Stickel 1992; Mikhail et al. 1999). Negative earnings

We thank Linda Bamber, Dan Givoly, and workshop participants at Arizona State University, Carnegie Mellon University, and the University of Illinois. This paper is partly based on a prior working paper. We thank Bill Baber, Sudipta Basu, Jim McKeown, Oliver Kim, and Doug Stevens, and conference participants at the Financial Economics and Accounting conference, the AAA Annual Meetings, and the EAA Annual Meetings, as well as workshop participants at the University of Cincinnati, University College Cork, Emory University, Louisiana State University, and the University of Maryland for comments on the earlier working paper. We thank I/B/E/S for providing the forecast data. Professor Byard gratefully acknowledges the financial support provided by a PSC-CUNY grant from the City University of New York (PSC-CUNY # 66597-00 35).

Editor's note: This paper was accepted by Dan Dhaliwal.

*Submitted August 2005
Accepted September 2007*

surprises, i.e., bad earnings news, represent more egregious forecasting failures than positive surprises because market prices plummet disproportionately when earnings fall short of the average forecast (e.g., Bartov et al. 2002; Skinner and Sloan 2002). Such negative earnings surprises not only adversely affect the wealth of analysts' investor clients, but also likely impair analysts' relationships with management because negative earnings surprises hurt managers' wealth and reputation (Richardson et al. 2004; Ke and Yu 2006). We posit that these types of earnings surprises, which we term "egregious forecasting failures," motivate analysts to develop or acquire relatively more private information in an effort to avoid future egregious forecasting failures. Additionally, because both large and negative earnings surprises are, on average, less persistent, investors' demand for analysts' information is likely to be higher after these types of earnings surprises. Consistent with this greater demand for analysts' information, Stickel (1989) reports that analysts supply more forecasts after relatively large or negative earnings surprises. More directly, we find that analysts reduce their individual forecast errors more after such egregious forecasting failures.

There are three reasons why we expect that egregious forecasting failures motivate analysts to increase their reliance on private information relative to common information. First, as already discussed, more egregious forecasting errors likely motivate analysts to work harder to repair their reputations and to distinguish themselves by incorporating relatively more private, i.e., unique, information in their forecasts. Second, firms that are more likely to have such egregious forecasting errors tend to issue fewer voluntary public disclosures (see Waymire 1985; Miller 2002), thus limiting the availability of public information. Third, egregious forecasting failures reflect earnings surprises that are, on average, less persistent (see Freeman and Tse [1992] for larger earnings surprises and Basu [1997] for negative earnings surprises), so analysts should place relatively less reliance on these less-persistent public earnings signals.

Our measure of the degree to which individual analysts' forecasts incorporate private information has its theoretical roots in Holthausen and Verrecchia's (1990) consensus concept. Specifically, we use the measure of analyst consensus developed by Barron et al. (1998) (hereafter BKLS Consensus). This measure, which is conceptually the same as Holthausen and Verrecchia's (1990) construct, estimates the ratio of common-to-total information in the average analyst's forecast.¹ Since analysts' total information is comprised of both common and private information, BKLS Consensus is also a measure of the *complement of the proportion of private information in individual analysts' forecasts*.

Using BKLS Consensus, we can therefore draw inferences about the degree to which analysts base their forecasts on private information relative to common information before and after earnings announcements. Similar to Barron, Byard, and Kim (2002), we find that, on average, BKLS Consensus decreases significantly after earnings announcements. This result suggests that the degree to which analysts' forecasts of future earnings incorporate private information relative to common information increases after earnings announcements. We extend their finding by demonstrating that analysts' reliance on private information increases more after relatively large or negative earnings surprises than after other earnings

¹ Holthausen and Verrecchia's (1990) measure is the degree to which investors' consensus about future dividends is reduced from an initial value of 1. Our BKLS Consensus measure is similar, except that we focus on the change in consensus among analysts about future earnings.

surprises.² We also demonstrate that after we control for the reduction in individual analyst forecast error, there is a greater decrease in the mean forecast error after relatively large or negative earnings surprises. This is because as analysts' reliance on private information increases, relatively more of their individual forecast errors are idiosyncratic and, thus, averaged out in the computation of the mean forecast (see BKLS Equation (14), p. 426).

Collectively, our results—together with Stickel's (1989) study—show that the analyst community responds to more egregious forecasting failures by: (1) decreasing the error in their individual forecasts of earnings more; (2) basing their individual forecasts *relatively* more on private information; and (3) issuing more forecasts. We show that each effect plays an incremental role in reducing error in the mean forecast.

Our results have implications for investors seeking more information after relatively large or negative earnings surprises. We show that after such egregious earnings surprises analysts develop or acquire relatively more private information and that analysts' forecast errors become more idiosyncratic, thus reducing the error in the mean forecast more. For investors who want the most accurate earnings expectation, our evidence shows that it is particularly important to observe the mean forecast, not just individual forecasts, after these types of earnings surprises. Our results also have implications for researchers wanting to understand when the analyst community most complements financial reports. For example, our results are of particular interest to researchers who want the least forecast error in their measures of the likely future earnings or the market's earnings expectation after egregious surprises. Finally, our results are also of interest to regulators concerned with the effect of an accounting policy choice on earnings volatility: our results show that a community of analysts will likely mitigate any effect of larger earnings surprises on the accuracy of expectations of future earnings.

We organize the paper in seven sections. Section II outlines the background and our hypotheses. Section III describes the research design and sample selection procedures. Section IV contains the results. Section V provides further analysis, and Section VI outlines our robustness analysis. Conclusions follow in Section VII.

II. BACKGROUND AND HYPOTHESES DEVELOPMENT

Background: Measuring the Degree to Which Individual Analysts' Forecasts Incorporate Private Information

The incremental usefulness of an individual analyst's forecast to investors is not determined simply by the amount of new private information it conveys but rather by the amount of new private information it conveys relative to the amount of information commonly conveyed by all forecasts. Given enough common information, more private information has little incremental usefulness. Thus, one measure of the incremental usefulness of each individual analyst's forecast is the proportion of private (relative to total) information it conveys.

The main result of BKLS shows that, under certain assumptions, one can express the ratio of common-to-total information in the average analyst forecast—BKLS Consensus (p), in terms of observable characteristics of analysts' earnings forecasts. Specifically, the

² BKLS Consensus may decrease after earnings announcements because analysts acquire or *develop* more private information. Analysts may *develop* new private information after an earnings announcement by using private information (perhaps acquired earlier) that is complementary to the public information contained in an earnings announcement, as described by Kim and Verrecchia (1994, 1997) and Barron, Byard, and Kim (2002, 824).

expected squared error in the mean forecast (SE), expected forecast dispersion (D), and the number of analysts' forecasts (N) is (from BKLS Proposition 1, p. 427):

$$\rho = \frac{\text{Common Forecast Error}}{\text{Average Total Error}} = 1 - D/V = \frac{\left(SE - \frac{D}{N}\right)}{\left(SE - \frac{D}{N}\right) + D} \quad (1)$$

where:

N = the number of forecasts;

D = the dispersion in analysts' forecasts, which is the sample variance of the analysts'

individual forecasts (F_i) around the mean forecast ($\bar{F}$), i.e., $\sum_{i=1}^N (F_i - \bar{F})^2 / (N - 1)$;

SE = the squared error in the mean forecast, calculated as the difference between the mean forecast ($\bar{F}$) and actual earnings (A), squared, i.e., $(A - \bar{F})^2$;

V = BKLS Uncertainty, the average squared error in individual forecasts, is measured as the mean squared difference between analysts' individual forecasts (F_i) and

actual reported earnings (A), i.e., $\sum_{i=1}^N (F_i - A)^2 / N$.

Under certain assumptions, we can interpret BKLS Consensus (ρ) as the average ratio of the common-to-total information in *all* analysts' forecasts. Barron, Byard, and Kim (2002, 839–844) argue empirically that these assumptions are a reasonable first approximation. Further, they point out that only two of these assumptions are necessary to use BKLS Consensus as a proxy for the consensus construct of Holthausen and Verrecchia (1990) and thus as a proxy for the degree of commonality of information impounded in the *average* forecast.

The first necessary assumption is that forecasts are unbiased, on average. Otherwise, a systematic bias in analyst forecasts will artificially inflate the common error in forecasts, making it appear as if analysts' forecasts incorporate more common—relative to total—information. The second necessary assumption is that managers do not engage in earnings management to meet or beat the mean forecast. Otherwise, such earnings management will artificially deflate the common error in forecasts, making it appear as if analysts' forecasts incorporate less common—relative to total—information. Section VI provides evidence supporting the robustness of our results to possible violations of these two assumptions.

Because BKLS Consensus (ρ) measures the ratio of common-to-total information in the average analyst forecast, it also represents the *complement* of the ratio of private-to-total information in the average analyst forecast. BKLS Consensus, thus, captures the relative importance analysts place on their common and private information. For example, a decrease in BKLS Consensus indicates that analysts base their forecasts less on common information and more on private information.

Hypotheses Development

As already discussed, there are a number of reasons why we expect analysts to increase their reliance on private (relative to common) information after egregious forecasting failures. First, egregious forecasting failures can adversely affect analysts. Thus, we expect

that after egregious forecasting failures, analysts strive to distinguish themselves and to decrease the error in their individual forecasts. They can do this by incorporating more private information in their forecasts. Second, firms that experience these types of earnings surprises are likely to make fewer voluntary public disclosures. Firms with more volatile earnings—that are more likely to have large earnings surprises—are less likely to issue voluntary public disclosures (Waymire 1985). Firms that are performing poorly—that are more likely to have negative earnings surprises—are also less likely to issue voluntary public disclosures (Miller 2002). This limits the public information analysts can use, further increasing their incentive to develop or acquire more private information.³ Third, prior research indicates that large earnings surprises and negative earnings surprises are less persistent (Freeman and Tse 1992; Basu 1997). This makes the commonly observed current earnings surprise less useful in predicting future earnings. Thus, investors' demand for information is likely to be greater after relatively large earnings surprises. Prior research shows that analysts are able to identify less persistent earnings surprises (Ali et al. 1992; Mest and Plummer 2000) and are motivated to forecast the more persistent components of earnings (Gu and Chen 2004). Thus, building on these prior findings, we expect that when forecasting earnings, analysts rely more heavily on their private information after earnings announcements that cause large or negative earnings surprises. For these three reasons, we predict that large earnings surprises and negative earnings surprises prompt analysts to acquire or develop relatively more private information about upcoming earnings. Our first two hypotheses are thus:

H1: Relatively large absolute earnings surprises lead to larger drops in BKLS Consensus after earnings announcements.

H2: Negative earnings surprises lead to larger drops in BKLS Consensus after earnings announcements.

If analysts rely more on private information, then their individual forecasts should become more heterogeneous. The BKLS model suggests that this increased heterogeneity is useful because it means that more of the idiosyncratic error contained in individual forecasts is averaged out in the computation of the mean forecast. More generally, the BKLS model suggests that mean forecast error can be decreased by any of the following (see BKLS Equation (14), p. 426):

- (1) Decreasing the error in individual forecasts, i.e., decreasing the average (squared) error in individual forecasts (ΔV);
- (2) Increasing the *proportion* of private information in each individual forecast so that analysts' individual forecast errors become more independent. Relatively more of these individual forecast errors are then averaged out of the mean forecast. This second effect is captured by the decrease in BKLS Consensus ($\Delta \rho$);⁴ or,
- (3) Increasing the number of forecasts, represented by N in the BKLS model. The larger the number of analysts' forecasts, the more completely idiosyncratic error is averaged out in determining the mean forecast.

³ However, there is some evidence that firms provide more public disclosure with earnings announcements that represent large surprises. Chen et al. (2002) show that firms are more likely to voluntarily include a balance sheet disclosure in an earnings announcement press release when the announced earnings number represents a relatively large earnings surprise.

⁴ BKLS Consensus must be less than 1 to derive any benefit from aggregating individual forecasts. If BKLS Consensus equals 1, i.e., if all analysts agree, then all individual forecasts are equal to the mean forecast and, thus, the mean forecast will not be more accurate than any individual forecast.

Our third hypothesis provides evidence on the BKLS model's implication that these three factors contribute to decreasing the error in the mean forecast. Specifically, we test whether the mean forecast error of subsequent earnings decreases more after relatively large or negative earnings surprises—two conditions associated with greater decreases in BKLS consensus, even after controlling for the average decrease in the error in the underlying individual forecasts (ΔV) and increases in the number of analysts forecasting (ΔN). Evidence supporting H3 below would demonstrate that the greater heterogeneity in analysts' forecasts following egregious forecasting failures reflects *private information*, not just noise:

H3: The mean forecast error decreases more after relatively large or negative earnings surprises, even after controlling for the average decrease in the error in individual forecasts and the change in the number of forecasts.

In summary, H3 tests whether the analyst community provides more accurate aggregate (mean) earnings forecasts after relatively large or negative earnings surprises, when there is likely to be increased investor demand for an accurate forecast.

III. RESEARCH DESIGN AND SAMPLE SELECTION

Research Design

We collect data on analysts' forecasts made soon before and after quarterly earnings announcements. For each quarterly earnings announcement q , we examine the change in BKLS Consensus regarding quarter $q+1$ earnings. To be included in our main sample, each firm-quarter q must have at least two individual analysts issuing forecasts of quarter $q+1$ earnings in the 45-day period immediately before the quarter q earnings announcement and who also revise their forecasts in the 30-day window immediately after the announcement. Matching individual analysts before and after a quarterly earnings announcement ensures that when we compute the change in BKLS Consensus we base our comparison on forecasts from the same set of individual analysts.

Using these matched forecasts of quarter $q+1$ earnings, we calculate *ex post* realized dispersion and squared error in the mean forecast, which we use as proxies for *expected* dispersion (D) and squared error in the mean forecast (SE). Estimates of *expected* dispersion (D), squared error in the mean forecast (SE), as well as the number of analysts producing new forecasts both before and after the earnings announcement (N), are substituted into Equation (1) to calculate the level of BKLS Consensus (ρ) both before (ρ^B) and after (ρ^A) the quarter q earnings announcement. We use these estimates to calculate the change in BKLS Consensus ($\Delta\rho$) around quarterly earnings announcements as follows: $\Delta\rho = \rho^A - \rho^B$. Since ρ is a measure of the incremental usefulness of individual analysts' forecasts in terms of decreasing the error in the mean forecast, $\Delta\rho$ measures the change in the incremental usefulness of individual analysts' forecasts after an earnings announcement.

We measure our primary explanatory variables using data from I/B/E/S and the Compustat quarterly databases. The absolute earnings surprise (*SURPRISE*) for quarter q is calculated as: $|Actual\ EPS - Mean\ EPS\ Forecast|/Stock\ Price$, where *Actual EPS* is actual quarter q earnings from the I/B/E/S database, *Mean EPS Forecast* is the mean forecast of quarter q earnings made in the 45-day period before the quarter q earnings announcement, and stock price is measured at the end of fiscal quarter q (about 45 days before the quarter q earnings announcement).⁵ A dummy variable (*BADNEWS*) equals 1 if *SURPRISE* is negative, and 0 otherwise.

⁵ Our results are unchanged if we use the median forecast to calculate *SURPRISE*.

We include the number of analysts matched before and after the quarter q earnings announcement (N) and firm size ($SIZE$), measured as market capitalization, as control variables. We include N as a control variable because Barron, Byard, and Kim (2002) find larger decreases in BKLS Consensus after earnings announcements where N is greater; thus, we expect N is negatively associated with $\Delta\rho$. We control for firm size because private information-based trading is likely to be more profitable in larger firms than in smaller firms because of larger firms' greater liquidity (Atiase 1985). Consistent with this idea, Byard (1998) finds that BKLS Consensus decreases faster for larger firms. We thus expect that $SIZE$ is negatively associated with $\Delta\rho$. Finally, we also include two sets of dummy variables to control for year (YR_DUM) and quarter (QTR_DUM) fixed effects.

Equation (2) below tests H1 and H2 regarding the cross-sectional determinants of the change in the relative importance of analysts' private information after earnings surprises, captured by the change in BKLS Consensus around the quarter q earnings announcement for firm i ($\Delta\rho_{iq}$):

$$\Delta\rho_{iq} = \alpha_0 + \alpha_1 SURPRISE_{iq} + \alpha_2 BADNEWS_{iq} + \alpha_3 N_{iq} + \alpha_4 SIZE_{iq} + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq}. \quad (2)$$

Hypothesis 1 predicts that $\alpha_1 < 0$, and H2 predicts that $\alpha_2 < 0$. That is, analysts' forecasts incorporate more private—relative to common—information after relatively large or negative earnings surprises.

To test H3, we regress the change in the mean forecast error of quarter $q+1$ earnings around the quarter q earnings announcement ($\Delta Error\_Mean_{iq}$) on both $SURPRISE$ and $BADNEWS$, while controlling for the number of analysts forecasting (N), the change in the average error in the underlying individual analysts' forecasts regarding quarter $q+1$ earnings (i.e., BKLS Uncertainty, ΔV), and firm size ($SIZE$). $\Delta Error\_Mean_{iq}$ is the change in the mean forecast error of quarter $q+1$ earnings, i.e., the absolute mean forecast error of quarter $q+1$ earnings after the quarter q earnings announcement minus the absolute mean forecast error of quarter $q+1$ earnings before the quarter q earnings announcement, scaled by stock price at the end of quarter q . ΔV is the change in BKLS Uncertainty—the change in the average (squared) error in individual forecasts of quarter $q+1$ earnings around the quarter q earnings announcement. We test H3 using the following equation:

$$\Delta Error\_Mean_{iq} = \beta_0 + \beta_1 SURPRISE_{iq} + \beta_2 BADNEWS_{iq} + \beta_3 N_{iq} + \beta_4 \Delta V_{iq} + \beta_5 SIZE_{iq} + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq}. \quad (3)$$

Recall that H3 predicts that, controlling for the number of forecasts N matched before and after an earnings announcement, the mean forecast error decreases more after relatively large or negative earnings surprises, *even after controlling for the decrease in the average error in individual forecasts* (ΔV).⁶ This arises because, as individual analysts rely more on private information, relatively more of their own forecast errors become idiosyncratic and

⁶ We do not control for ΔV in our Equation (2) because its purpose is to examine the change in the ratio of common-to-total information (BKLS Consensus) and not the reduction in the mean forecast error.

thus get averaged out in the computation of the mean forecast.⁷ H3 thus predicts that larger values of *SURPRISE* are associated with larger decreases in $\Delta Error\_Mean$, so we expect $\beta_1 < 0$. Hypothesis 3 also predicts that there are larger decreases in $\Delta Error\_Mean$ after negative earnings surprises, so we expect $\beta_2 < 0$.

Sample Selection

Our sample includes all firm-quarters between 1986 and 2003 meeting the following data requirements:

- (1) The earnings announcement date for quarter q is available from Compustat;
- (2) Data are available from the I/B/E/S Detail file for at least two individual analysts who issue a forecast of quarter $q+1$ earnings in the 45-day period before the quarter q earnings announcement date and who also revise these forecasts in the 30-day period after the quarter q earnings announcement;
- (3) Actual earnings for quarter $q+1$ are available from the I/B/E/S database;
- (4) Data are available to calculate the surprise in quarter q earnings, i.e., at least one forecast of quarter q earnings is available from the 45-day period immediately before the quarter q earnings announcement date, and actual earnings for quarter q is available from the I/B/E/S database; and,
- (5) Data for common shares outstanding (data item #61) and stock price at the end of quarter q (data item #14) are available from the Compustat quarterly file.

Using the Compustat quarterly database, we first select firm-quarters with data for quarterly earnings announcement dates for quarter q and data to measure market capitalization at the end of quarter q . We match this with I/B/E/S data measuring the size of the earnings surprise for quarter q as well as the change in BKLS Consensus (about quarter $q+1$ earnings) around the quarter q earnings announcement (using a minimum of two forecasts of quarter $q+1$ earnings made before and after the quarter q earnings announcement date).⁸ This yields a sample of 17,752 firm-quarters. The I/B/E/S treatment of stock splits may create a bias in the measurement of BKLS Consensus. After splits, I/B/E/S rounds forecasts to the nearest penny. This rounding may create more zero errors in the mean forecast (see Payne and Thomas 2003), which would bias BKLS Consensus for these observations toward zero. To avoid this potential measurement error we use the I/B/E/S Unadjusted data (the pre-split adjusted data).⁹

IV. RESULTS

Descriptive Statistics

Table 1 outlines the descriptive statistics for our sample of 17,752 firm-quarters. Consistent with the results of Barron, Byard, and Kim (2002) and Barron et al. (2005), we find that, on average, BKLS Consensus decreases after earnings announcements. The mean (median) change in BKLS Consensus is -0.043 (-0.001), both of which are significantly less than zero ($p < 0.001$, one-tailed). Because BKLS Consensus is the ratio of common-to-total information in the average analyst forecast, it is also the complement of the proportion of private information in the average analyst forecast. These results, thus, indicate

⁷ We cannot test H3 by regressing $\Delta Error\_Mean$ on Δp as such a test would be strictly mechanical (see BKLS Equation (14), p. 426).

⁸ The results are robust to using a minimum of three (or five) analysts matched before and after quarter q earnings announcements.

⁹ Our inferences are, however, unchanged using the regular split-adjusted I/B/E/S data.

TABLE 1
Descriptive Statistics

Variable	Mean	One-Tailed p-value for t-test of H_0 that $\Delta p = 0$	Median	One-Tailed p-value for Sign Test H_0 that $\Delta p = 0$	25th Percentile	75th Percentile	Standard Deviation
Δp	-0.043	(<0.001)	-0.001	(<0.001)	-0.209	0.111	0.568
$\Delta Error\_Mean$	-0.001	(<0.001)	-0.002	(<0.001)	-0.002	0.000	0.015
<i>SURPRISE</i>	0.004		0.001		0.000	0.003	0.036
<i>BADNEWS</i>	0.280		0.000		0.000	1.000	0.450
<i>N</i>	3.481		3.000		2.000	4.000	2.453
ΔV	-0.029		-0.002		-0.029	0.018	1.464
<i>SIZE</i>	5,213.95		1,131.80		362.30	3,655.94	14,953

Descriptive statistics for the entire sample of 17,752 firm-quarters (1986–2003). All p-values (shown in parentheses) are for one-tailed tests of significance.

- Δp = change in BKLS Consensus around the quarter q earnings announcement, calculated using N forecasts of quarter $q+1$ earnings matched before and after the quarter q earnings announcement. Using these matched forecasts, we calculate BKLS Consensus before (p^B) and after (p^A) the quarter q earnings announcement using the model of Barron et al. (1998). The change in BKLS Consensus is then: $\Delta p = p^A - p^B$;
- $\Delta Error\_Mean$ = change in the absolute mean forecast errors around the quarter q earnings announcement, calculated using N forecasts of quarter $q+1$ earnings matched before and after the quarter q earnings announcement. Using these matched forecasts, we calculate the change in the mean forecast error as the error in the mean forecast after the announcement minus the error in the mean forecast before the announcement, scaled by stock price at the end of the quarter q ;
- SURPRISE* = earnings surprise, calculated as $|Actual\ EPS - Mean\ EPS\ Forecast| / Stock\ Price$, where actual and forecasted EPS are for quarter q and are from the I/B/E/S database. Stock price is at the end of fiscal quarter q . The mean forecast is calculated as the mean of the forecasts of quarter q earnings made in the 45-day period before the quarter q earnings announcement;
- BADNEWS* = dummy variable equal to 1 if the earnings surprise in quarter q is negative (i.e., if quarter q I/B/E/S actual earnings is less than the mean I/B/E/S forecast), and 0 otherwise;
- N = number of individual (matched) analysts who issue a forecast of quarter $q+1$ earnings in the 45-day period before the quarter q earnings announcement and who also revise this forecast in the 30-day period immediately after the quarter q earnings announcement. A minimum of two (matched) individual analysts' forecasts before and after the quarter q earnings announcement date are required;
- ΔV = change in BKLS Uncertainty around the quarter q earnings announcement calculated using forecasts of quarter $q+1$ earnings (N above) matched before and after the quarter q earnings announcement. Using these matched forecasts, we calculate BKLS Uncertainty before (V^B) and after (V^A) the quarter q earnings announcement using the model of Barron et al. (1998). The change in BKLS Uncertainty is then: $\Delta V = V^A - V^B$; and
- SIZE* = market capitalization at the end of quarter q , calculated as: (Common states outstanding at the end of quarter q * Closing price at end of the quarter). These are data items #61 and #14 from the Compustat quarterly file, respectively. Market capitalization for all firm-quarters is deflated to 1982 CPI-dollars using CPI data from the Department of Labor.

that the proportion of private information in the average forecast *increases* after earnings announcements. Consistent with our expectations, untabulated descriptive statistics indicate that there are larger decreases in BKLS Consensus after relatively large or negative earnings surprises. Mean Δp for the largest (smallest) decile of *SURPRISE* is -0.064 (-0.005), and the mean Δp for negative (positive) earnings surprises is -0.068 (-0.029). Similarly, the mean forecast error also tends to decrease significantly after earnings announcements. Mean (median) $\Delta Error\_Mean$ is -0.001 (-0.002). Again, both are significantly less than zero ($p < 0.001$, one-tailed).

As Table 1 shows, the mean (median) price-scaled absolute earnings surprise (*SURPRISE*) is 0.004 (0.001). *BADNEWS*, a dummy variable equal to 1 for negative earnings surprises, indicates that 28 percent of the firm-quarter observations have negative earnings surprises. We match two or more individual analysts' forecasts before and after each earnings announcement. Table 1 shows that mean *N* is 3.481 analysts. As we expect, BKLS Uncertainty (*V*), the average squared error in individual forecasts, typically decreases after earnings announcements. Mean (median) ΔV is -0.029 (-0.002). The descriptive statistics for *SIZE* indicate that, on average, our sample firms are large. Mean (median) market capitalization is \$5.2 billion (\$1.1 billion).

Correlation Analysis

Table 2 reports Spearman correlations among our dependent variables— Δp and $\Delta Error\_Mean$, our primary explanatory variables, *SURPRISE* and *BADNEWS*, and our control variables—*N*, *SIZE*, and ΔV . We show results for decile rank specifications, that is, where observations are ranked on the respective variable and assigned ranks from 0 to 9 based upon the deciles of the distribution of the respective variable. This mitigates the influence of extreme values and facilitating a comparison of the magnitudes of coefficients. However, our inferences are robust to using alternative specifications (i.e., raw values, or full ranks).

As we expect, our two dependent variables, Δp and $\Delta Error\_Mean$, are significantly positively correlated ($p < 0.001$, one-tailed). We expect a positive correlation here because, based upon the intuition of the BKLS model, larger decreases in BKLS Consensus are

TABLE 2
Correlation Analysis, Spearman Rank Correlations Using Decile Ranks^a

	Δp (p-value) ^b	$\Delta Error\_Mean$ (p-value) ^b	<i>SURPRISE</i> (p-value) ^b	<i>BADNEWS</i> (p-value) ^b	<i>N</i> (p-value) ^c	ΔV (p-value) ^c	<i>SIZE</i> (p-value) ^c
Δp	1.00	0.44 (<0.001)	-0.04 (<0.001)	-0.06 (<0.001)	-0.042 (<0.001)	0.37 (<0.001)	-0.01 (0.03)
$\Delta Error\_Mean$		1.00	-0.21 (<0.001)	-0.18 (<0.001)	-0.069 (<0.001)	0.62 (<0.001)	0.22 (<0.001)
<i>SURPRISE</i>			1.00	0.23 (<0.001)	-0.079 (<0.001)	-0.15 (<0.001)	-0.29 (<0.001)
<i>BADNEWS</i>				1.00	0.005 (0.900)	-0.13 (<0.001)	-0.11 (<0.001)
<i>N</i>					1.00	-0.131 (<0.001)	0.214 (<0.001)
ΔV						1.00	0.02 (<0.001)
<i>SIZE</i>							1.00

Correlation analysis for the variables used in Equations (2) and (3). The sample size is 17,752 firm-quarters (1986–2003). The endnotes of Table 1 define all variables.

^a The original data are ranked into decile portfolios (0 to 9). We estimate correlations using these decile ranks.

^b One-tailed tests of significance.

^c Two-tailed tests of significance.

expected to be associated with larger decreases in *Error_Mean*. Consistent with our hypotheses, our two primary explanatory variables, *SURPRISE* and *BADNEWS*, are both significantly negatively correlated with $\Delta\rho$ ($p < 0.001$, one-tailed) and with $\Delta\text{Error\_Mean}$ ($p < 0.001$, one-tailed).

Consistent with prior studies (e.g., Abarbanell and Lehavy 2003), our results indicate that negative earnings surprises tend, on average, to be larger: *SURPRISE* and *BADNEWS* are significantly positively correlated ($p < 0.001$, one-tailed). Consistent with Barron, Byard, and Kim (2002) and Byard (1998), our control variables in Equation (2), *N* and *SIZE*, are significantly negatively correlated with $\Delta\rho$ ($p < 0.03$, two-tailed). This suggests that the ratio of common-to-total information decreases more for firms followed by more analysts and for larger firms. *N* is also significantly negatively related to $\Delta\text{Error\_Mean}$ ($p < 0.001$, two-tailed). On the other hand, *SIZE* is significantly positively correlated with $\Delta\text{Error\_Mean}$ ($p < 0.001$, two-tailed). Consistent with the underlying intuition of the BKLS model (see BKLS, Proposition 1, p. 426), $\Delta\text{Error\_Mean}$ is significantly positively correlated with ΔV and with $\Delta\rho$ ($p < 0.001$, two-tailed for both). This suggests that the mean forecast error decreases when uncertainty (i.e., error in individual forecasts) is reduced or when the ratio of common-to-total information in the average forecast is reduced.

The correlations between our primary explanatory variables, *SURPRISE* and *BADNEWS*, and our control variables, *N*, ΔV , and *SIZE*, are also consistent with the results of prior studies, (e.g., Lang and Lundholm 1996). *N* is negatively correlated with *SURPRISE*, indicating that firms with heavier analyst following have smaller earnings surprises. Both *SURPRISE* and *BADNEWS* are significantly negatively correlated with the change in the error in individual forecasts (ΔV), suggesting a greater reduction in individual forecast errors after large or negative surprises.¹⁰ *SIZE* is significantly negatively correlated with both *SURPRISE* and *BADNEWS* ($p < 0.01$, two-tailed) consistent with prior evidence that larger firms have smaller earnings surprises (Bamber 1986), and fewer negative earnings surprises (Matsumoto 2002). This evidence—that the control variables are associated with both our independent and dependent variables—highlights the importance of controlling for *N* and *SIZE*, an omission that could cause correlated omitted variable problems. Because our study design matches analysts before and after earnings surprises, the negative correlation between *SURPRISE* and *N* does not address whether larger earnings surprises prompt analysts to issue more forecasts—the third effect that could decrease the mean forecast error. We use an alternative sample including all forecasts to explore this issue.

Regression Analyses

We use a decile rank regression specification, i.e., ordinary least squares (OLS) regressions based upon decile ranks of the data. This relaxes the linearity assumption and assumes only a monotonic relationship between the dependent and independent variables (see Bradshaw et al. 2001; Lang and Lundholm 1996).¹¹ However, our inferences are robust to using raw values, regular rank regression, or dummy variable specifications, i.e., creating a dummy equal to 1 if $\Delta\rho < 0$ as the dependent variable. We report p-values based upon Rogers' (1993) method to compute standard errors. This method allows for heteroscedasticity and correlation among observations for the same firms.

¹⁰ Brown and Han (1992) find that more surprising earnings lead to smaller reductions in *forecast dispersion*. Barron et al. (2006), however, show that changes in forecast dispersion proxy much more for changes in BKLS Consensus ($\Delta\rho$) than for changes in BKLS Uncertainty (ΔV).

¹¹ In using rank regression specifications, we also avoid making implicit assumptions regarding the functional form of analysts' loss function (see Basu and Markov 2004).

Table 3 first reports the results for Equation (2), which tests H1 and H2. The results are consistent with our predictions that *SURPRISE* (-0.043 , $p < 0.001$, one-tailed) and *BADNEWS* (-0.205 , $p < 0.001$, one-tailed) are both significantly negatively related to $\Delta\rho$. These results support our expectation that more surprising earnings (H1) and negative earnings surprises (H2), i.e., more egregious forecasting failures, are associated with larger decreases in BKLS Consensus and, thus, an increase in analysts' reliance on private information. Consistent with the results of prior studies (Barron, Byard, and Kim 2002; Byard 1998), both control variables, *N* and *SIZE*, are significantly negatively related to $\Delta\rho$, suggesting that the ratio of common-to-total (private-to-total) information decreases (increases) more for firm with higher analyst following or higher market capitalization. Since we estimate our regressions using decile rank regressions, the larger coefficients on *SURPRISE* and *BADNEWS* compared to *SIZE* indicate that a one-decile change in *SURPRISE* and *BADNEWS* has a greater impact on $\Delta\rho$ than a one-decile change in *SIZE*.

The first set of results reported in Table 3 shows that individual analysts decrease (increase) the ratio of common-to-total (private-to-total) information in their forecasts more after relatively large or negative earnings surprises than after other earnings surprises. Given

TABLE 3
Decile Rank Regression Analysis^a

Regression analysis of cross-sectional relationship between change in BKLS Consensus ($\Delta\rho$) and the change in the mean forecast error ($\Delta Error\_Mean$) around quarterly earnings announcements and the magnitude and sign of quarterly earnings surprises. The sample size is 17,752 firm-quarters (1986–2003).

$$\Delta\rho_{iq} = \alpha_0 + \alpha_1 SURPRISE_{iq} + \alpha_2 BADNEWS_{iq} + \alpha_3 N_{iq} + \alpha_4 SIZE_{iq} + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq} \quad (2)$$

$$\Delta Error\_Mean_{iq} = \beta_0 + \beta_1 SURPRISE_{iq} + \beta_2 BADNEWS_{iq} + \beta_3 N_{iq} + \beta_4 \Delta V_{iq} + \beta_5 SIZE_{iq} + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq} \quad (3)$$

Dependent Variable	Intercept (Predicted) Coeff. (p-value) ^b	<i>SURPRISE</i> (Predicted) Coeff. (p-value) ^b	<i>BADNEWS</i> (Predicted) Coeff. (p-value) ^b	<i>N</i> (Predicted) Coeff. (p-value) ^b	ΔV (Predicted) Coeff. p-value) ^b	<i>SIZE</i> (Predicted) Coeff. (p-value) ^b	Adj. R^2
$\Delta\rho$	5.211 (0.004)	(–) –0.043 (<0.001)	(–) –0.205 (<0.001)	(–) –0.045 (<0.001)		(–) –0.022 (<0.001)	0.01
$\Delta Error\_Mean$	6.933 (<0.001)	(–) –0.073 (<0.001)	(–) –0.476 (<0.001)	(–) –0.040 (<0.001)	(+) 0.585 (<0.001)	0.180 (<0.001)	0.44

^a The original data are ranked into decile portfolios (0 to 9); these decile ranks are used in the regression analysis. Results are robust to using alternative rank regression specifications (e.g., using raw ranks).

^b p-value is for one-tailed (two-tailed) tests of significance where the sign is predicted (unpredicted), based on Rogers' (1993) method to compute standard errors. This method allows for heteroscedasticity and correlation between observations for the same firms.

YR_DUM_y = dummy variable equal to 1 for year *y*, and 0 otherwise; and
QTR_DUM_q = dummy variable equal to 1 for quarter *q*, and 0 otherwise.

The remaining variables are defined in Table 1.

this result, H3 predicts that for a given number of analysts' forecasts matched before and after an earnings announcement N —even after controlling for the average change in the error in analysts' individual forecasts (ΔV)—*SURPRISE* and *BADNEWS* are associated with a further decrease in mean forecast error ($\Delta Error\_Mean$). However, if the greater heterogeneity of analysts' forecasts after relatively large or negative earnings surprises primarily reflects noise and not useful private information, then after controlling for ΔV , one would not expect *SURPRISE* and *BADNEWS* to be associated with the decrease in $\Delta Error\_Mean$.

The second set of results reported in Table 3 tests H3. We find that, even after controlling for N , ΔV , and *SIZE*, both *SURPRISE* and *BADNEWS* are significantly negatively related to $\Delta Error\_Mean$ ($p < 0.001$, one-tailed for both). That is, consistent with H3, both *SURPRISE* and *BADNEWS* are associated with a decrease in the error in the mean forecast, even after controlling for the average decrease in the error in individual forecasts. We argue that this arises because egregious forecasting failures (relatively large or negative earnings surprises) motivate individual analysts to incorporate relatively more private information in their forecasts. Thus, their individual forecast errors become more idiosyncratic, and relatively more of these individual forecast errors average out of the mean forecast.¹² The results for the control variables are intuitive. The statistically significant ($p < 0.001$, two-tailed) negative coefficient on N indicates that there is a greater decrease in the error in the mean forecast when there is a greater number of analysts forecasting both before and after the earnings announcement; the statistically significant ($p < 0.001$, two-tailed) positive coefficient on ΔV indicates that there is a greater decrease in the error in the mean forecast when there is a larger decrease in the average error in individual analysts' forecasts. *SIZE* is significantly positively associated with $\Delta Error\_Mean$ ($p < 0.001$, one-tailed) indicating that larger firms tend to have smaller decreases in $\Delta Error\_Mean$.¹³

Egregious Forecasting Failures and the Change in the Number of Forecasts

Table 3 reports results based on our main sample where N individual analysts' forecasts are matched before and after quarterly earnings announcements. We know, however, that there is an increase in the number of forecasts immediately after earnings announcements (Stickel 1989). As a result, it is unclear whether our results regarding the change in BKLS Consensus, Δp , generalize to the full set of observable forecasts after an earnings announcement or whether our results apply only to the subset of frequently updating analysts who issue forecasts both before and after earnings announcements. To probe this question, we construct a new sample using the same 17,752 quarterly earnings announcements. For this more general sample we include all analysts' forecasts. We do not limit the sample to just those frequently updating analysts who issue forecasts both shortly before and shortly after earnings announcements. We replicate our analysis using this more general sample, which we term the *total analyst sample*. Table 4 displays these results.

First, consistent with the results of Stickel (1989), we find that there tend to be more analysts' forecasts after quarterly earnings announcements than before these earnings announcements. The mean (median) increase in the number of analysts' forecasts, ΔN , is 3.471 (3). Thus, in the *total analyst sample*, the mean number of forecasts before (after) earnings announcements is 3.481 (6.952). We re-estimate Equations (2) and (3) using this

¹² Our inferences are unchanged if we use a dummy variable coded 1 (0) for increases (decreases) in $\Delta Error\_Mean$ and estimate Equation (3) as a logit model.

¹³ The significant positive association between *SIZE* and $\Delta Error\_Mean$ arises because *SIZE* is positively correlated with ΔV (see Table 2). Specifically, while larger firms tend to have more analysts issuing forecasts (larger N) and larger decreases in BKLS Consensus (Δp), two effects which lead to larger decreases in $\Delta Error\_Mean$, this is more than offset by the fact that larger firms tend to have smaller decreases in V .

TABLE 4
Supplemental Analysis Using the Total Analyst Sample

Table 3 reports results using our main sample of 17,752 firm-quarters (1986–2003) where we match at least two individual analysts who issue forecasts both before and after an earnings announcement. We also constructed an alternative sample for the same 17,752 firm-quarters, but where we do *not* match individual analysts' forecasts before and after earnings announcements. We term this unmatched analyst sample the *total analyst sample*. This table reports results for the same cross-sectional analysis using this alternative sample. Similar to Table 3, this analysis also employs decile rank regressions.^a

$$\Delta p_{iq} = \alpha_0 + \alpha_1 SURPRISE_{iq} + \alpha_2 BADNEWS_{iq} + \alpha_3 \Delta N_{iq} + \alpha_4 SIZE_{iq} + \sum_{y=1987}^{y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq} \quad (2a)$$

$$\Delta Error\_Mean_{iq} = \beta_0 + \beta_1 SURPRISE_{iq} + \beta_2 BADNEWS_{iq} + \beta_3 \Delta N_{iq} + \beta_4 \Delta V_{iq} + \beta_5 SIZE_{iq} + \sum_{y=1987}^{y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq} \quad (3a)$$

Equations (2a) and (3a) are the same as Equations (2) and (3), except the change in the number of analysts' forecasts (ΔN) is used instead of the number of analysts' forecasts (N) as a control variable. ΔN is the change in the number of individual analysts' forecasts made after the earnings announcement compared to the number made before the earnings announcement. On average, there are more forecasts issued or revised after the earnings announcement: mean (median) ΔN is 3.471 (3). So, in this *total analyst sample* that uses unmatched analysts the average number of analysts' forecasts before (after) the quarterly earnings announcement is 3.481 (6.952).

Dependent Variable	Intercept (Predicted) Coeff. (p-value) ^b	SURPRISE (Predicted) Coeff. (p-value) ^b	BADNEWS (Predicted) Coeff. (p-value) ^b	ΔN (Predicted) Coeff. (p-value) ^b	ΔV (Predicted) Coeff. (p-value) ^b	SIZE (Predicted) Coeff. (p-value) ^b	Adj. R ²
Δp	4.469 (<0.001)	(-) -0.030 (<0.001)	(-) -0.280 (<0.001)	(-) -0.062 (<0.001)		(-) -0.019 (0.026)	0.02
$\Delta Error\_Mean$	7.692 (<0.001)	(-) -0.071 (<0.001)	(-) -0.435 (<0.001)	(-) -0.028 (<0.001)	(+) 0.696 (<0.001)	0.162 (<0.001)	0.57

^a The original data are ranked into decile portfolios (0 to 9); these decile ranks are used in the regression analysis. Results are robust to using alternative rank regression specifications (e.g., using raw ranks).

^b p-value is for one-tailed (two-tailed) tests of significance where the sign is predicted (unpredicted), based on Rogers' (1993) method to compute standard errors; this method allows for heteroscedasticity and correlation between observations for the same firms.

YR_DUM_y = dummy variable equal to 1 for year y , and 0 otherwise; and

QTR_DUM_q = dummy variable equal to 1 for quarter q , and 0 otherwise.

The remaining variables are defined in Table 1.

more general sample but substitute ΔN for N (we label these new Equations (2a) and (3a) in Table 4). The results in Table 4 are similar to those in Table 3 using our main sample of matched analysts. Both *SURPRISE* and *BADNEWS* are significantly negatively associated with both Δp and $\Delta Error\_Mean$ ($p < 0.001$, one-tailed). Our results, thus, apply to the full set of analysts' forecasts that are available after earnings announcements and are not attributable solely to a subset of frequently updating analysts. Finally, the significant negative relation between ΔN and $\Delta Error\_Mean$ ($p < 0.001$, two-tailed) is intuitive, indicating that

there is a greater reduction in the error in the mean forecast if one can aggregate a greater number of individual analysts' forecasts.

In an untabulated supplementary analysis we confirm Stickel's (1989) evidence, suggesting that ΔN increases with *SURPRISE* and *BADNEWS*. This is important because, as we reiterate again, the mean forecast error can be reduced by: (1) decreasing the average (squared) error in individual forecasts, i.e., BKLS Uncertainty, ΔV , (2) decreasing the ratio of common-to-total information in individual analysts' forecasts, i.e., BKLS Consensus, $\Delta \rho$, or (3) increasing the number of forecasts, ΔN . Thus, our evidence indicates that *SURPRISE* and *BADNEWS* are linked to all three effects that serve to decrease the error in the mean forecast.

V. FURTHER ANALYSIS

The Informational Components of Decreases in BKLS Consensus

To better understand why there are larger decreases in BKLS Consensus after egregious forecasting failures, we re-estimate Equation (2) but substitute as the dependent variable estimates of the percentage changes in the BKLS information precision measures, the precision of analysts' common (h) and private (s) information (see Barron, Byard, and Kim 2002, 428).¹⁴ Since BKLS Consensus measures the ratio of common-to-total information in the average analyst forecast, BKLS Consensus changes when the percentage changes in h and s differ.

This analysis helps differentiate between two alternative scenarios that could explain our results regarding the change in BKLS Consensus. In the first scenario, analysts' forecasts are more heterogeneous after relatively large or negative earnings surprises because there are larger percentage increases in the precision of analysts' private information (s) after relatively large or negative earnings surprises. Alternatively, BKLS Consensus, ρ , could also decline if the percentage increase in the precision of analysts' common information (h) is *decreasing* in *SURPRISE* and *BADNEWS*. In other words, it is possible that analysts' forecasts contain a higher proportion of private information after relatively large or negative earnings surprises simply because the percentage increase in the precision of analysts' common information is lower after these types of earnings surprises.

As Table 5 shows, the results from re-estimating Equation (2) with the percentage changes in h and s as dependent variables yield results consistent with the first scenario above.¹⁵ While the percentage change in both h and s are positively related to *SURPRISE* and *BADNEWS*, there is a stronger relationship between both *SURPRISE* and *BADNEWS* and $\% \Delta s$ than with $\% \Delta h$. To formally test this assertion, we estimate both the $\% \Delta s$ and $\% \Delta h$ regressions simultaneously as a Seemingly Unrelated Regression (SUR) estimation that allows for a comparison of parameter estimates across equations. This SUR estimate indicates that both *SURPRISE* and *BADNEWS* are significantly more positively associated with $\% \Delta s$ than with $\% \Delta h$ ($p \leq 0.001$, two-tailed, for both). In summary, there are significantly larger percentage increases in the precision of analysts' private information after more egregious forecasting failures.¹⁶

¹⁴ Three additional assumptions are required for the BKLS precision measures h and s : (1) analysts' information is either idiosyncratic or common; (2) forecast errors are normally distributed; and (3) individual forecasts are of homogenous precision (see Barron, Byard, and Kim 2002).

¹⁵ Consistent with Barron, Byard, and Kim (2002), we find that on average the percentage changes in s and h after earnings announcements are both significantly positive ($p < 0.001$, two-tailed).

¹⁶ One possible source of this private information after earnings announcements could be complementary private information "triggered" by earnings announcements; that is, private information of the type described by Kim and Verrecchia (1994, 1997).

TABLE 5
Seemingly Unrelated Regression (SUR) Estimates and Comparison of the Percentage Change in the Precision of Analysts' Common (*h*) and Private (*s*) Information

SUR estimates of the cross-sectional relationship between the percentage changes in BKLS information precision measures—*h* and *s*—around quarterly earnings announcements, and the magnitude and sign of the quarterly earnings surprise. The SUR estimation also provides for a comparison of the strength of the relationship between our primary explanatory variables—*SURPRISE* and *BADNEWS*—and both $\% \Delta h$ and $\% \Delta s$. The sample size is 17,752 firm-quarters (1986–2003).

$$\% \Delta h_{it} \text{ or } \% \Delta s_{it} = \alpha_0 + \alpha_1 SURPRISE_{it} + \alpha_2 BADNEWS_{it} + \alpha_3 N_{it} + \alpha_4 SIZE_{it} \\ + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{it}$$

Dependent Variable	Intercept (Predicted) Coeff. (p-value) ^a	<i>SURPRISE</i> (Predicted) Coeff. (p-value) ^a	<i>BADNEWS</i> (Predicted) Coeff. (p-value) ^a	<i>N</i> (Predicted) Coeff. (p-value) ^a	<i>SIZE</i> (Predicted) Coeff. (p-value) ^a	Adj. R ²
$\% \Delta h$	3.620 (<0.001)	(+) 0.016 (0.053)	(+) 0.114 (0.025)	(+) 0.169 (<0.001)	−0.040 (<0.001)	0.002
$\% \Delta s$	3.539 (<0.001)	(+) 0.042 (<0.001)	(+) 0.418 (<0.001)	(+) 0.112 (<0.001)	0.002 (0.784)	0.020
Two-tailed p-value for comparison of coefficients across $\% \Delta h$ and $\% \Delta s$ regressions		(<0.001)	(0.001)			

^a p-value is for one-tailed (two-tailed) tests of significance where the sign is predicted (unpredicted), based upon Rogers' (1993) method to compute standard errors; this method allows for heteroscedasticity and correlation between observations for the same firms.

^b The original data are ranked into decile portfolios (0 to 9); these decile ranks are then used in the regression analysis. The results are robust using alternative rank regression specifications (e.g., using raw ranks).

$\% \Delta h$ = percentage change in the precision of analysts' common information after the quarterly earnings announcement;

$\% \Delta s$ = the percentage change in the precision of analysts' private (i.e., idiosyncratic) information after the quarterly earnings announcement;

YR_DUM_y = a dummy variable equal to 1 for year *y*, and 0 otherwise; and

QTR_DUM_q = a dummy variable equal to 1 for quarter *q*, and 0 otherwise.

The remaining variables are defined in Table 1.

Confirming Firm-Level Evidence Concerning the Effect of Earnings Persistence

Our analysis is partly motivated by the argument that analysts place more importance on their private information after less persistent earnings surprises. We test whether there are larger decreases in BKLS Consensus after relatively large or negative earnings surprises partly because these two factors are associated with less persistent earnings surprises (Freeman and Tse 1992; Basu 1997). While it is not possible to directly measure the persistence of individual earnings surprises, it is possible to measure the average persistence of a firm's earnings surprises. To further explore our expectation regarding the association between the persistence of earnings surprises and changes in BKLS Consensus around earnings surprises, we conduct a firm-level analysis using a common measure of firms'

earnings persistence, *PERSISTENCE*, defined as the AR(1) coefficient of a regression of income for year t on income for year $t-1$, i.e., the coefficient b in the following regression: $Income_t = a + b \cdot Income_{t-1} + \xi_t$ (e.g., Francis and Smith 2005). This model is estimated for each firm i separately. Consistent with prior studies (e.g., Francis and Smith 2005), we define *Income* as operating income after depreciation and amortization (Compustat #178). We then estimate the following model:

$$\Delta p_{iq} = \phi_0 + \phi_1 PERSISTENCE_i + \phi_2 N_{iq} + \phi_3 SIZE_{iq} + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq} \quad (4)$$

For comparability, we include the same control variables in Equation (4) as in Equation (2). Table 6 confirms our argument. *PERSISTENCE* is significantly positively related to the change in BKLS Consensus ($p = 0.001$, one-tailed). BKLS Consensus measures the ratio of common-to-total information in the average forecast, which is also the *complement* of the weight that analysts place on their private information. The results thus confirm that, on average, analysts place more importance on the private information they develop or acquire after the earnings announcements of firms that tend to have less persistent earnings innovations. These results are also robust to including a set of industry dummy control variable for each of the four-digit SIC codes represented in the sample.

TABLE 6
Further Analysis,
An Alternative Measure of Earnings Persistence

Regression analysis of cross-sectional relationship between change in BKLS Consensus around quarterly earnings announcements and earnings persistence. The sample size is 17,752 firm-quarters (1986–2003).

$$\Delta p_{iq} = \phi_0 + \phi_1 PERSISTENCE_i + \phi_2 N_{iq} + \phi_3 SIZE_{iq} + \sum_{y=1987}^{Y=2003} \eta_y YR\_DUM_y + \sum_{q=2}^{Q=4} \pi_q QTR\_DUM_q + \varepsilon_{iq} \quad (4)$$

Dependent Variable	Intercept (Predicted) Coeff. (p-value) ^a	<i>PERSISTENCE</i> (Predicted) Coeff. (p-value) ^a	<i>N</i> (Predicted) Coeff. (p-value) ^a	<i>SIZE</i> (Predicted) Coeff. (p-value) ^a	Adj. R ²
Δp	(?) 4.623 (<0.0001)	(+) 0.025 (=0.001)	(-) -0.448 (<0.001)	(-) -0.035 (<0.275)	0.01

^a p-value is for one-tailed (two-tailed) tests of significance where the sign is predicted (unpredicted), based on Rogers' (1993) method to compute standard errors; this method allows for heteroscedasticity and correlation between observations for the same firms. This analysis is also conducted using decile rank regressions; however, the results are robust using alternative rank regression specifications (e.g., using raw ranks).

PERSISTENCE is a firm-specific measure of the average persistence of a firm's earnings. It is calculated as the AR(1) coefficient (b) in a firm-specific regression of *Income* in year t on *Income* in year $t-1$, i.e., $Income_t = a + b \cdot Income_{t-1} + \xi_t$. This equation is estimated for each firm in the sample separately. Consistent with prior studies of earnings persistence (e.g., Francis and Smith 2005), we defined *Income* as operating income after depreciation and amortization expense. The remaining variables are defined in Tables 1 and 3.

VI. ROBUSTNESS TESTS

Is the Decrease in BKLS Consensus Due to the Industry Composition of Our Sample?

The results of Barron, Byard, Kile, and Riedl (2002) show that the level of BKLS Consensus is lower for firms in certain high-tech industries. Using the same SIC codes that Barron, Byard, Kile, and Riedl (2002) use to identify high-tech industries, we remove firms in these industries from our sample and re-run our analysis. Our inferences are unaffected.

Including the Pre-Announcement Level of BKLS Consensus (ρ^B) as a Control Variable

We are interested in whether analysts themselves decrease ρ because they develop or acquire more private information, and not whether ρ decreases simply because there is more potential for this to happen. Thus, we also re-estimate Equations (2) and (3) including the level of pre-announcement BKLS Consensus (ρ^B) regarding quarter $q+1$ earnings as an additional control variable. This controls for the possibility that the decrease in BKLS Consensus will tend to be larger when there is a higher level of pre-announcement BKLS Consensus. The untabulated results indicate that our inferences are unaffected when we include ρ^B as another control variable.

The Effect of Regulation Fair Disclosure

We also test for the possible effect of Regulation Fair Disclosure (Reg FD) on our results. Reg FD, which went into effect on October 23, 2000, prohibits managers from disclosing material information to select analysts without simultaneously making the information available to other analysts and the general investing public. In untabulated results, we find that estimating Equations (2) and (3) separately for the pre- and post-FD periods yields almost identical results for both subperiods. Pooling the pre- and post-FD periods and creating a dummy variable equal to 1 (0) for the post-FD (pre-FD) period, then interacted with *SURPRISE* and *BADNEWS*, yields the same inference. The results for the interaction terms are statistically insignificant.¹⁷ These results suggest that neither our results, nor those of Barron, Byard, and Kim (2002), are driven by private communication between management and select analysts before Reg FD took effect.

Robustness of Results to Possible Violation of BKLS Assumptions

Finally, we assess the robustness of our results to potential violations of the two assumptions necessary to interpret BKLS Consensus (ρ) as the ratio of common-to-total information in the average forecast, and thus as the *complement* of the average degree to which analysts base their forecasts on private information (see Barron, Byard, and Kim 2002). The first assumption is that analysts' forecasts are unbiased. As already discussed, a systematic bias in analyst forecasts will artificially inflate the common error in forecasts, making it appear as if there has been more commonality in the information upon which analysts base their forecasts. The second necessary assumption is that actual earnings are not conditional on forecasted earnings, i.e., managers do not engage in earnings management to meet the mean forecast. If managers engage in earnings management to meet the mean forecast, then this may artificially deflate the common error in forecasts, making it appear as if there has been less commonality in the information analysts based their forecasts upon. Note that since we use forecasts of quarter $q+1$ earnings to measure BKLS

¹⁷ We dropped the year dummy variables in this analysis because the FD dummy variable would be collinear with the year dummies.

Consensus, a violation of a BKLS assumption would arise if earnings management drives actual earnings for quarter $q+1$ to meet the mean analyst forecast.

Prior studies, however, show patterns in the cross-sectional distribution of analyst forecast errors that are consistent with both an optimistic bias in forecasts, and earnings management to meet or beat the mean forecast. Management has incentives to “meet or beat” analyst earnings forecasts (Bartov et al. 2002), and studies suggest that managers may manage earnings to do so (see Dechow et al. 2003; Burgstahler and Eames 2006).¹⁸ Additionally, Abarbanell and Lehavy (2003) suggest that the optimistic bias in forecasts arises because of firms’ earnings management, and they show that forecast optimism is due to a small number of very large negative forecast errors, which they suggest may indicate earnings management to take a “big bath” (see also Gu and Wu 2003).¹⁹

To further test the possible effect of analysts’ optimistic bias on our analysis, we split our sample into two subsamples and conduct our analysis for both subperiods. While early studies document that, in general, analysts’ forecasts tend to be optimistically biased, recent research indicates that this phenomenon is much less evident from the early 1990s onward (e.g., see Brown 2001). In untabulated analysis we split our sample period into subperiods 1986 to 1993, and 1994 to 2003. The results are similar for both subperiods. Recent research also indicates that forecast optimism is much less evident in forecasts made closer to earnings announcement dates (Richardson et al. 2004). This is important because our sample period comprises only forecasts of quarter $q+1$ earnings made in the last six months before the quarter $q+1$ earnings announcement dates. Consistent with a lack of bias in these short-term earnings forecasts, we find that 49 (51) percent of the mean forecasts of quarter $q+1$ earnings used to calculate p^A in our sample are optimistic (pessimistic).

For our first set of robustness tests, we use Abarbanell and Lehavy’s (2003) evidence to identify earnings forecasts for quarter $q+1$ that are most likely to be affected by two types of earnings management: “big baths” that cause *ex post* forecast optimism and earnings management to meet-or-beat the mean forecast. First, we assume that the observations where the actual quarter $q+1$ earnings meet or beat the mean forecast from the 45-day window before the quarter $q+1$ earnings announcement by 5 cents or less are most likely to have been affected by this earnings management.²⁰ Second, we identify the most likely cases of big baths as observations where the quarter $q+1$ earnings forecast error is within the bottom 2 percent of the distribution of price-scaled forecast errors for quarter $q+1$, where the $q+1$ forecast errors are calculated using the mean forecast from the 45-day pre-announcement window. We drop these potentially tainted observations and re-run our analysis. Our inferences are unchanged.

We undertook two additional tests that further examine the robustness of our results to the possible presence of earnings management to meet or beat the mean forecast. First, we use an alternative sample of forecasts: we replicate all our analysis using forecasts of two-quarters-ahead earnings (i.e., forecasts of quarter $q+2$ earnings issued before and after

¹⁸ Durtschi and Easton (2005), however, find that the discontinuities around zero may not necessarily reflect earnings management.

¹⁹ A “big bath” refers to the practice whereby management focuses many accounting write-offs in a single reporting period. The evidence of Abarbanell and Lehavy (2003) and Gu and Wu (2003) suggest that analysts do not fully anticipate these infrequent events.

²⁰ As a further robustness test, we also test the sensitivity of our results to alternative cutoff points for which we defined the observations that are most likely to have been affected by earnings management to meet-or-beat analysts’ earnings forecasts as those where the quarter $q+1$ earnings are between 0 and 1 (3) cents above the mean forecast. Our inferences are unchanged using these alternative cutoff points. In addition, our inferences are unchanged if we use the median forecast from the 45-day period prior to the $q+1$ earnings announcement date.

quarter q earnings announcements). That is, we use forecasts made at earlier points in time before the eventual earnings announcement dates. If actual earnings are managed to meet a forecast, then it is most likely going to occur just prior to the announcement date (see Bartov et al. 2002). Thus, by using earlier forecasts, which are less likely to be the target of earnings management, the results of this analysis further demonstrate the robustness of our results to the presence of this type of earnings management. Our inferences are unchanged using this alternative forecast sample.

Second, we attempt to identify and control for firms that *ex ante* have a stronger incentive to manage earnings to meet or beat analysts' forecasts. Kasznik and McNichols (2002) show that the market premium is higher for firms that consistently meet or beat analysts' earnings forecasts.²¹ Therefore, firms that have continuously met or beaten analysts' forecasts, say in the previous four quarters, likely have a stronger incentive to manage earnings to meet or beat analysts' forecasts in the current quarter. Based on this argument, we create a dummy variable equal to 1 for firm-quarters when firms have continuously met or beaten analysts' forecasts in the four most recent quarters. We find our inferences are unchanged when we incorporate this additional control variable.²²

In conclusion, our inferences remain unchanged using alternative sample periods, control variables, and specifications to test for the impact of possible violations of the BKLS assumptions that are required to interpret BKLS Consensus, ρ , as a measure of the ratio of common-to-total information in analysts' forecasts, and thus, as a *complement* of the weight analysts place on their private information.

VII. DISCUSSION AND CONCLUSIONS

This study provides evidence suggesting that financial analysts base their forecasts of future earnings more on private information after earnings surprises that represent more egregious forecasting failures, i.e., after relatively large or negative earnings surprises. There are three reasons why we expect that egregious forecasting failures motivate analysts to increase their reliance on private information relative to common information. First, analysts work harder to develop or acquire new private information to avoid the adverse reputational effects of large earnings surprises (e.g., Stickel 1992; Mikhail et al. 1999). Second, firms that are likely to suffer more egregious forecasting errors tend to issue fewer voluntary public disclosures (see Waymire 1985; Miller 2002), thus limiting the availability of public information. Third, egregious forecasting failures reflect earnings surprises that are, on average, less persistent (Freeman and Tse 1992; Basu 1997), so analysts will place relatively less reliance on these less-persistent public earnings signals. Investors' demand for analysts' private information is also likely higher after these egregious earnings surprises (Stickel 1989).

We use the measure of analyst consensus Barron et al. (1998) (BKLS Consensus) develop as our measure of the degree to which analysts rely on private versus common information. BKLS Consensus is the ratio of common-to-total information in the average analyst forecast. Consistent with our expectation, we find that BKLS Consensus decreases more after relatively large or negative earnings surprises, indicating that analysts develop more private information after relatively large or negative earnings surprises than after other earnings surprises. Further analyses using the BKLS measures of the precision

²¹ Similarly, Lopez and Rees (2000) show that the premium in three-day abnormal returns for meeting or beating analysts' forecasts is greater if a firm also met or beat analysts' forecasts in each of the four prior quarters.

²² The results of Skinner and Sloan (2002) also indicate that firms with high market-to-book ratios have a stronger incentive to meet-or-beat analysts' forecasts and, as a result, are likely to have a stronger incentive to engage in earnings management to do so. Controlling for the market-to-book ratio does not affect our inferences.

of analysts' common and private information confirm that the proportion of private information in forecasts increases more after relatively large or negative earnings surprises because there is a greater percentage increase in the precision of analysts' private information after these types of earnings surprises compared to other earnings surprises. Because we show that the degree to which BKLS Consensus changes around earnings announcements varies with characteristics of the earnings announcement, our results suggest that the relative importance of analysts' informational role with respect to earnings announcements—providing substitute versus complementary private information—varies across different types of earnings announcements.

According to the BKLS model, the mean forecast error can be decreased in three ways: (1) decreasing the error in analysts' individual forecasts; (2) increasing the *proportion* of private information in each individual forecast; and (3) increasing the number of forecasts. The consensus measure of the BKLS model (BKLS Consensus) captures the second effect. When BKLS Consensus decreases, analysts base their forecasts more on private information, and analysts' individual errors become more idiosyncratic. Relatively more of these individual forecast errors are then aggregated out of the mean forecast, which decreases the error in the mean forecast. Consistent with this intuition, controlling for the change in the number of forecasts and the decrease in the average error in individual forecasts, we show that there is a greater decrease in the mean forecast error after egregious forecasting failures. We also find that the average (squared) error in individual forecasts decreases more after relatively large or negative earnings surprises, and we confirm Stickel's (1989) result that analysts provide more forecasts after relatively larger or negative earnings surprises. In addition, we confirm that these two effects also serve to decrease the error in the mean forecast. Our evidence thus suggests that all three effects discussed above contribute toward decreasing the error in the mean forecast more after egregious forecasting failures.

A lack of BKLS Consensus reflects information asymmetry between relatively informed versus uninformed investors (see Botosan et al. 2004; Barron et al. 2005; Barron et al. 2006), so our results suggest that information asymmetry is likely exacerbated after large or negative earnings surprises. This is consistent with prior evidence that firms that avoid relatively large or negative earnings surprises enjoy a lower cost of capital (Botosan et al. 2004). It also suggests a rationale for the observed market premium for firms that consistently meet or beat analysts' expectations (Kasznik and McNichols 2002).

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Audit Quality and Properties of Analyst Earnings Forecasts

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ABSTRACT: Under the assumption that audit quality relates positively to unobservable financial reporting quality, we investigate whether audit quality is associated with the predictability of accounting earnings by focusing on analyst earnings forecast properties. The evidence shows that analysts' earnings forecast accuracy is higher and the forecast dispersion is smaller for firms audited by a Big 5 auditor. We further find that auditor industry specialization is associated with higher forecast accuracy and less forecast dispersion in the non-Big 5 auditor sample but not in the Big 5 auditor sample. Overall, our results suggest that high-quality audit provided by Big 5 auditors and industry specialist non-Big 5 auditors is associated with better forecasting performance by analysts.

Keywords: *audit quality; analysts' forecasts; forecast accuracy and dispersion; industry specialization.*

Data Availability: *All analyses are based upon publicly available data.*

I. INTRODUCTION

In this study, we investigate the association between audit quality (proxied by auditor size and industry specialization) and properties of analysts' annual earnings forecasts (i.e., accuracy and dispersion of the forecasts). Prior studies have largely relied on stock price to infer how market participants evaluate audit quality. However, Holthausen and Watts (2001) note that empirical evidence based on the association between accounting characteristics and stock price often says little about whether financial statement users actually include the accounting numbers in their decision-making processes. We take a more direct approach by examining whether more reliable accounting earnings due to higher

We thank an anonymous reviewer and Dan Dhaliwal (editor) for helpful suggestions to improve the manuscript. We have also received useful comments from Chih-Ying Chen, Paul Dunmore, Andrew Ferguson, Bin Ke, Young Kwon, Ariel Markelevich, Chul Park, and seminar participants at Singapore Management University, University of Sydney, 2006 AFAANZ Conference in Wellington, New Zealand, and the 2006 AAA Annual Meeting in Washington, D.C.

Editor's note: The paper was accepted by Dan Dhaliwal.

*Submitted July 2006
Accepted August 2007*

quality audits are associated with an improvement in users' decision-making by focusing on the properties of earnings forecasts issued by financial analysts. To the extent that high-quality audits increase reporting reliability by reducing both intentional and unintentional measurement errors in historical earnings (Becker et al. 1998; Watkins et al. 2004), which analysts use to predict future earnings, their forecasts are likely more accurate and less dispersed when audit quality is high.

This research relates to the prior audit quality and analyst earnings forecast literatures, and it is primarily motivated by the following facts. First, while there is some evidence that users' perception of financial reporting reliability increases with audit quality (e.g., Teoh and Wong 1993; Krishnan 2003; Balsam et al. 2003; Khurana and Raman 2004), there is little evidence on how the users make better decisions when audit quality is high. Second, while previous research suggests that security analysts use financial statement information in formulating forecasts (e.g., Abarbanell and Bushee 1997; Brown et al. 1987; Lang and Lundholm 1996), no study examines whether audit quality is associated with analysts' forecasting ability. Our study attempts to fill these voids in the literature.

Financial statements are a primary source of information in capital markets, which depend on high-quality information to function properly. External audits contribute to financial reporting reliability by providing an independent assessment of the accuracy and fairness with which financial statements represent the results of operations, financial position, and cash flows in conformity with generally accepted accounting principles (e.g., Abdel-khalik and Solomon 1988).¹ Assuming audit quality improves financial reporting reliability, we empirically test whether (1) analysts' consensus forecasts become more accurate and (2) analysts' forecast dispersion decreases, with audit quality.

Similar to Titman and Trueman (1986), we define a high-quality audit as an audit that improves the reliability of financial statement information and allows investors to make a more precise estimate of the firm's value.² We operationalize audit quality using auditor size, Big 5 versus non-Big 5 (e.g., Chung and Lindsay 1988; Teoh and Wong 1993; DeFond and Jambalvo 1993; DeFond et al. 2000; Choi and Wong 2007), and the degree of an auditor's industry specialization (e.g., Balsam et al. 2003; Beasley and Petroni 2001; Craswell et al. 1995; Owchoso et al. 2002) to identify high-quality audit service.³

Our empirical evidence yields several findings. First, forecast accuracy is higher (lower) and the dispersion is lower (larger) when a firm hires a Big 5 (non-Big 5) auditor. In addition, earnings forecasts for the clients of industry specialist non-Big 5 auditors are more accurate and less dispersed than those for the clients of non-industry specialist non-Big 5 auditors. However, the forecasts for the clients of industry specialist Big 5 auditors are neither more accurate nor less dispersed than those for the clients of non-industry specialist Big 5 auditors.

This study contributes to the literature in several important aspects. First, we provide evidence that suggests that earnings are more predictable when audit quality is high. To our knowledge, our study is the first to demonstrate such association. By documenting this association, it extends the line of literature showing that financial statement audits have

¹ Information in financial statements is reliable if it is: representationally faithful (i.e., corresponds to that which it purports to measure); verifiable (i.e., provides assurance that some consensus in the reported measurement exists); and free from bias. We argue that higher quality audits yield more reliable accounting numbers, including the summary measure, earnings.

² Similarly, Wallace (1980) defines audit quality as a measure of the auditor's ability to reduce noise and bias and improve fineness in accounting data. An alternative definition of audit quality is the joint probability that an existing problem is detected and reported (DeAngelo 1981).

³ We use the term "Big 5" for the former Big 6 and their predecessor/successor auditors.

significant economic consequences in capital markets (e.g., Teoh and Wong 1993; Khurana and Raman 2004). Next, by identifying an important cross-sectional determinant of analyst forecast accuracy, our study has implications for accounting academics who use the forecasts as a proxy for the capital markets' earnings expectation when examining the information content of accounting information. Our findings should also be useful to investors who use these forecasts to value firms, as it suggests that the earnings expectations of firms that employ a high-quality auditor are likely to be more accurate.⁴

The remainder of this paper is organized as follows. In the next section, we develop empirically testable hypotheses. In Section III, we describe the research design and variable measurement. In Section IV, we describe the sample, discuss the results, and describe our sensitivity analyses. We then provide concluding comments in Section V.

II. RELATED STUDIES AND HYPOTHESES DEVELOPMENT

Related Prior Studies

Previous studies have examined the market's perception of audit quality. Teoh and Wong (1993) argue that Big 5 auditors in fact provide higher quality audit service than non-Big 5 auditors. Indeed, they find that the earnings response coefficients (ERCs) of Big 5 clients are significantly higher than those of non-Big 5 clients. Krishnan (2003) finds that market returns are more closely associated with the discretionary accruals of clients of Big 5 auditors than clients of non-Big 5 auditors, consistent with the idea that large auditors enhance the reliability of reported accruals. Regarding specialist auditors, Balsam et al. (2003) document that discretionary accruals of industry specialist auditors are smaller than those of non-specialist auditors. Similarly, Khurana and Raman (2004) suggest that investors' perception of financial reporting quality (as captured in *ex ante* cost of equity) increases with perceived audit quality. However, the question of whether audit quality is associated with an actual improvement in financial statement users' decision making remains to be answered.

Prior studies have also examined various determinants of analysts' forecast properties. Brown et al. (1987) claim that the accuracy of an earnings forecast depends on the difficulty or complexity of the forecasting task. Lang and Lundholm (1996) find that analysts' forecasts are more accurate, less dispersed, and exhibit less volatile revisions for large firms. They also find that the forecasts are less accurate for firms with large earnings surprises. Hwang et al. (1996) find that forecasts are less accurate for loss-reporting firms. Kross et al. (1990) report that security analysts' earnings forecast accuracy is inversely related to earnings volatility. Eames and Glover (2003) document associations between the earnings level and both forecast error and earnings predictability. However, no prior study examines the potential association between audit quality and the accuracy and dispersion of analysts' annual earnings forecasts.

Hypotheses Development

In general, auditor monitoring can improve the quality of accounting earnings by minimizing the difference between a client's reported economic circumstances and the unobservable underlying economic situation of the client (e.g., Wallace 1980). More specifically, a user would likely consider a (perceived) high-quality audit reliable because it reduces both intentional and unintentional reporting errors. Indeed, Becker et al. (1998) indicate that high-quality audits decrease earnings management (i.e., managers' intentional reporting

⁴ For instance, Schipper (1991) notes that security analyst earnings forecasts are often used as a proxy for an unobservable market's earnings expectations and often used to value firms.

bias) and Watkins et al. (2004) suggest that unintentional measurement errors could be reduced by high-quality audits.

Our maintained assumption in this study is that financial reporting reliability increases with audit quality, which we infer from auditor size and industry specialization. Brand name auditors are viewed as providing higher quality audits based on their perceived competence (by virtue of their heavy spending on auditor training facilities and programs) and independence (by virtue of their size and large portfolio of clients, which presumably gives them the financial strength to stand up to, or walk away from, a client if necessary) (Beatty 1989). An industry specialist, in addition to a brand name, is known to offer a higher level of assurance than does a non-specialist (e.g., O'Keefe et al. 1994; Craswell et al. 1995; Beasley and Petroni 2001; Owghoso et al. 2002).

Previous literature suggests that analysts, a sophisticated group of financial statement users, use historical earnings information in predicting firms' future earnings. For example, Schipper (1991) notes that analysts assimilate and process publicly available information such as past earnings and prices. Abarbanell and Bushee (1997) find that historical earnings explain variation in analysts' forecast revisions. Thus, it is likely that analysts' forecasting ability increases with the quality (i.e., reliability) of financial information they use to predict future earnings; the more the historical earnings information contains errors that are not indicative of the underlying economic situation or future firm performance, the less likely that analysts issue accurate forecasts. Following this line of reasoning, we predict that audit quality (measured using auditor size and industry specialization) is associated with analysts' earnings forecast accuracy, and formulate our first hypothesis as follows (stated in alternate form):

H1: A higher quality audit is positively associated with analysts' earnings forecast accuracy.

Analysts' forecast dispersion reflects uncertainty about the firm's information environment (e.g., Imhoff and Lobo 1992; Payne and Robb 2000). For example, Imhoff and Lobo (1992) interpret forecast dispersion as a proxy for *ex ante* earnings uncertainty (i.e., uncertainty about earnings before they are announced).⁵ Herrmann and Thomas (2005) support the information-deficit situation, noting that greater dispersion indicates less agreement among analysts due to the inability or unwillingness of some analysts to fully and objectively gather and process information. They further suggest that analysts with more precise information regarding future earnings are more likely to be in agreement, and thus the forecast dispersion should be smaller. Thus, to the extent that audit quality and financial reporting reliability are positively correlated, we predict that analysts' forecast dispersion will be negatively associated with audit quality (stated in alternate form):⁶

H2: A higher quality audit is negatively associated with analysts' earnings forecast dispersion.

⁵ Similarly, Lipe (1990) uses the standard deviation of residuals from a first-order autoregressive process of past earnings as a measure of predictability/uncertainty of earnings.

⁶ It is possible that our accuracy test results are at least in part driven by the fact that future earnings are more reliable when this period's audit quality is high. However, the dispersion tests are not subject to this issue because they do not depend on realized earnings.

III. EMPIRICAL DESIGN AND VARIABLE MEASUREMENT

Auditor size (*AUDITOR*) is coded 1 for a Big 5 auditor, and 0 otherwise. Regarding the significance of auditor size, DeAngelo (1981) and Datar et al. (1991) claim that large and more prestigious public accounting firms concerned about protecting their investment in reputation capital have more incentive than do other auditors to supply a high-quality audit. Further, Craswell et al. (1995) note that, although all public accounting firms must comply with minimum professional standards, the Big 5 firms voluntarily invest in higher levels of expertise and have incentives to provide higher quality audits to protect their reputations. Overall, these studies generally suggest that audit quality is likely to be positively related to audit firm size. Likewise, DeFond and Jiambalvo (1993) provide evidence consistent with the view that Big 5 auditors provide higher quality audits. Craswell et al. (1995), Francis and Krishnan (1995), and Francis and Reynolds (2000) find that the large audit firms have brand-name reputation, charge higher audit fees, and/or behave qualitatively differently from smaller audit firms. Palmrose (1988) also documents that large audit firms are involved in less litigation activity than smaller audit firms.

We compute industry specialization (*ISPEC*) by using the sum of the square root of the total assets of clients that an auditor has in a particular industry divided by the sum of the square root of the total assets of all clients of the auditor (Choi and Doogar 2005).⁷ In order to derive a more objective (i.e., externally valid) industry specialization measure, we use the entire population of Compustat firms to calculate the *ISPEC* variable.⁸ Industry specialist auditors are expected to provide higher-quality audit services than non-specialist auditors (Balsam et al. 2003; Beasley and Petroni 2001). Indeed, Owosho et al. (2002) show that industry experienced auditors are better able to detect errors within their industry specialization than outside their specialization. Also, O'Keefe et al. (1994) document that compliance with auditing standards is higher for industry specialist auditors than for non-specialists. Thus, the clients of the industry specialized auditors may have forecasts that are more accurate and less dispersed. We further expect that this tendency will be more prominent for the clients of non-Big 5 auditors. Big 5 auditors have a broad client base and various experience in different industries. In contrast, non-Big 5 auditors have a smaller client base and are relatively less experienced, which leads us to predict that auditors' industry specialization will matter more for the forecasts for non-Big 5 clients than for Big 5 clients.

To empirically test H1 and H2, we use the following Equations (1) and (3), respectively, which control for previously identified determinants of analysts' forecast properties.⁹ Firm size (*SIZE*), Zmijewski's (1984) financial distress score (*ZMIJ*), forecast horizon

⁷ To classify each industry, we use the first two digits of the SIC code. We classify all auditors that have fewer than 10 clients per year as non-specialist (*ISPEC* = 0) in order to remove the effect of tiny auditors who have only a few clients. Changing the cutoff value to 15, 12, or 8 does not alter our empirical results.

⁸ We use this measure because it represents the relative industry expertise of both Big 5 and non-Big 5 auditors. Later we examine the sensitivity of our finding to several alternative industry specialization measures. First, we calculate the square root of sales or the number of clients for the calculation of *ISPEC* rather than the square root of the total assets of clients. Although they are not tabulated for simplicity, we obtain similar results. The results are consistent with those reported in Table 5 if we use the square root of the clients' sales, but if we use the number of clients, *ISPEC* becomes insignificant. We also try a dichotomous variable based on whether the auditor is the industry leader (in terms of the market share) or not (e.g., Balsam et al. 2003). A downside of this measure is that it tends to classify a Big 5 (non-Big 5) auditor as an expert (a non-expert), and therefore it is difficult to disentangle the effects of industry specialization from auditor size. Presumably due to this reason, none of the market share-based measures of industry specialization in Balsam et al. (2003) load significantly in our empirical tests.

⁹ For simplicity, we omit firm and time subscripts.

(*HORIZON*), and earnings volatility (*STDROE*) are the common control variables across the two equations. Because auditor choice is an endogenous decision for each client and the factors that determine the auditor choice can also influence the dependent variables used in this study, we calculate the inverse Mills ratio (*INVMR*) and include it in all three models in order to control for the possible endogeneity in the estimations.¹⁰ In addition, to control for the presence of heteroscedasticity, we apply White's (1980) heteroscedasticity consistent standard errors for all regression analyses performed in this study. Furthermore, we apply the firm-clustering technique for all the analyses because multiple observations from the same firm (but from different years) are included in our dataset. We use the following Equation (1) to test H1:

$$\begin{aligned} ACCY = & \varphi_0 + \varphi_1 AUDITOR + \varphi_2 ISPEC + \varphi_3 (AUDITOR \times ISPEC) + \varphi_4 SIZE \\ & + \varphi_5 SURPRISE + \varphi_6 LOSS + \varphi_7 ZMIJ + \varphi_8 HORIZON + \varphi_9 STDROE \\ & + \varphi_{10} NANA + \varphi_{11} EL + \varphi_{12} INVMR + year\ dummies + \varepsilon. \end{aligned} \quad (1)$$

In Equation (1), forecast accuracy (*ACCY*) is measured by the negative of the absolute value of forecast error scaled by stock price at time $t-1$, following Lang and Lundholm (1996). We denote by $FORECAST_t^{t-1}$ the mean I/B/E/S consensus forecast of period t earnings made during the period starting two months before the corresponding actual earnings announcement and ending three days before the announcement; by EPS_t we denote actual earnings per share before extraordinary items at time t , taken from I/B/E/S, and by $PRICE_{t-1}$ the stock price at the end of period $t-1$. We remove the influence of stale forecasts by using only the most recent forecast for the calculation of the mean if an analyst announces multiple forecasts during the period. Then, forecast accuracy is defined as:¹¹

$$ACCY_t = (-1) \frac{|FORECAST_t^{t-1} - EPS_t|}{PRICE_{t-1}}. \quad (2)$$

Among independent variables, auditor choice (*AUDITOR*) and the degree of auditor industry specialization (*ISPEC*) are the measures of audit quality used in this study. We also include an interaction term between the two ($AUDITOR \times ISPEC$) to determine whether the effect of auditor industry specialization on analyst forecast accuracy varies with auditor size. We expect industry specialization to have less (more) impact on the audit quality of Big 5 (non-Big 5) auditors given that Big 5 (non-Big 5) auditors tend to have a

¹⁰ For this purpose, we first run a probit regression for auditor choice model that is similar to the model used by Choi and Wong (2007). The model is:

$$AUDITOR = \varphi_0 + \varphi_1 SIZE + \varphi_2 CAPINT + \varphi_3 INVREC + \varphi_4 LEVE + \varphi_5 LOSS + \varphi_6 ROA + \varphi_7 ISSUE.$$

In this regression, *AUDITOR* is the Big 5 indicator variable, *SIZE* is the log market value of equity, *CAPINT* is the capital intensity measured by long-term assets over total assets, *INVREC* is the inventory and receivables over total assets, *LEVE* is the total liabilities over total assets, *LOSS* is the loss indicator variable, *ROA* is return on assets, and *ISSUE* is the dummy variable for long-term debt issuance of more than 20 percent of existing long-term debt for the past two years. Using the results of this first-stage regression, we compute the inverse Mills ratio. In the first stage regression, all independent variables except *ISSUE* are significant at 1 percent level and pseudo- R^2 of the regression model is 0.2660.

¹¹ Observations having *ACCY* variable smaller than -1.5 (about 1 percent of the distribution) are removed as outliers. Our results are not sensitive to different outlier control approaches.

larger (smaller) client base and more (less) experience. Accordingly, we expect the coefficient on the interaction to be negative.

Firm size (*SIZE*) and number of analysts following (*NANA*) are included based on Lang and Lundholm (1996), who document a positive association between firm size, analyst following, and forecast accuracy. Absolute value of the earnings surprise (*SURPRISE*) is also based on Lang and Lundholm (1996), who find that larger changes in earnings are associated with less accurate forecasts. The loss indicator variable (*LOSS*) is included based on Hwang et al. (1996), who find that analysts' forecasts for loss-reporting firms are on average less accurate than forecasts for profit-reporting firms. Zmijewski's (1984) financial distress score (*ZMIJ*) is also included because financially distressed firms tend to have less accurate forecasts. Per Brown (2001), we control for forecast horizon (*HORIZON*), which is the natural logarithm of the average number of calendar days between the forecast announcement date and corresponding actual earnings announcement date. It is expected that a forecast announced closer to the actual earnings announcement date (i.e., short forecast horizon) is more accurate than a forecast announced in the earlier period (i.e., long forecast horizon). Earnings volatility (*STDROE*) is included based on Kross et al. (1990), who have shown that analysts' earnings forecasts are less accurate for firms with higher long-term earnings volatility. Because Eames and Glover (2003) report that earnings level is related to forecast accuracy, we include the earnings per share variable (*EL*).¹²

Second, to test H2, we use the following Equation (3).

$$\begin{aligned} DISP = & \varphi_0 + \varphi_1 AUDITOR + \varphi_2 ISPEC + \varphi_3 (AUDITOR \times ISPEC) + \varphi_4 SIZE \\ & + \varphi_5 SURPRISE + \varphi_6 ZMIJ + \varphi_7 HORIZON + \varphi_8 STDROE + \varphi_9 INVMR \\ & + year\ dummies + \epsilon. \end{aligned} \quad (3)$$

The dispersion of analysts' forecasts (*DISP*) is defined as the standard deviation of earnings forecasts issued by individual analysts. This variable has commonly been discussed as a measure for uncertainty about future earnings since it represents the consensus among analysts regarding future firm prospects (see, for instance, Imhoff and Lobo 1992; Barron and Stuerke 1998).¹³

$$DISP_t = \frac{STD(FORECAST_t)}{PRICE_{t-1}}. \quad (4)$$

Here, the control variables are similar to the ones in the accuracy model (i.e., Equation (1)), but we do not include number of analysts (*NANA*), loss dummy (*LOSS*), and earnings level (*EL*) because the conceptual link between these variables and forecast dispersion is not clearly established in the literature.¹⁴ We expect that large (*SIZE*) firms would have smaller dispersions, while financially distressed (*ZMIJ*) firms, firms with longer forecast horizon (*HORIZON*), and firms with more volatile earnings streams (*SURPRISE* and *STDROE*) would have larger dispersions.

¹² We winsorize *ZMIJ* and *EL* at 5 and -5 to mitigate the influence of outliers.

¹³ To draw a meaningful inference, we require that the firm is followed by at least four analysts for the purpose of computing *DISP*. We drop 14 observations that have *DISP* variable greater than 1.5 as outliers. Again, winsorizing at 1.5 does not appear to affect the empirical results.

¹⁴ Our results do not change if we include these three variables in the dispersion model.

IV. SAMPLE AND RESULTS

Sample Characteristics

The sample consists of U.S. firms with available data for six years from 1996 to 2001. Because our measure of forecast accuracy requires future years' data (i.e., year $t+1$ and $t+2$ data) on earnings, however, we use up to year 2003 data. Our final sample consists of 9,261 firm-year observations that have necessary analysts' forecast data available from I/B/E/S and financial data available from Compustat. We retain 3,749 firm-year observations for the forecast dispersion sample due to further data requirements.

Descriptive statistics of the regression variables are provided in Table 1. The mean (median) forecast accuracy (*ACCY*) is -0.030 (-0.003) in the sample, suggesting that the mean (median) difference between analysts' forecasts and corresponding actual earnings is about 3 (0.3) percent of the lagged stock price. The mean dispersion (*DISP*) of 0.021

TABLE 1
Descriptive Statistics

Variable	Mean	Median	Standard Deviation	1st Percentile	99th Percentile
<i>ACCY</i>	-0.030	-0.003	0.113	-0.606	0
<i>DISP</i>	0.021	0.002	0.094	0.000	0.485
<i>AUDITOR</i>	0.890	1.000	0.313	0.000	1.000
<i>ISPEC</i>	0.042	0.037	0.038	0.000	0.129
<i>SIZE</i>	5.486	5.544	2.335	0.020	11.031
<i>SURPRISE</i>	0.152	0.029	0.518	0.000	2.986
<i>LOSS</i>	0.359	0.000	0.480	0.000	1.000
<i>ZMIJ</i>	-1.765	-1.956	1.920	-4.971	5.000
<i>HORIZON</i>	2.9540	2.9444	0.4636	1.6094	3.9120
<i>NANA</i>	1.069	1.099	0.917	0.000	3.219
<i>STDROE</i>	0.147	0.002	0.097	0.000	0.503
<i>EL</i>	0.426	0.382	1.641	-2.271	3.081
<i>INVMR</i>	0.749	0.769	0.047	0.658	0.819

Variable Definitions:

ACCY = accuracy in analysts' earnings forecasts, defined as the negative of the absolute difference between the forecast and actual earnings, scaled by price;

DISP = dispersion in analysts' earnings forecasts;

AUDITOR = dummy variable that takes the value of 1 (0) if the firm is affiliated with a Big 5 (non-Big 5) auditor;

ISPEC = industry specialist auditor measure, defined as the sum of the square root of the total assets of the clients of an auditor in a specific industry divided by total sum of the square root of the total assets of entire clients of the auditor;

SIZE = logarithm of market value of equity;

SURPRISE = this year's earnings minus last years' earnings deflated by stock price;

LOSS = coded as 0 (1) for firm-year observations with positive (negative) earnings;

ZMIJ = Zmijewski's financial distress score;

HORIZON = log of the average of the number of calendar days between mean forecast announcement date and subsequent actual earnings announcement date;

NANA = log of number of analysts following the client;

STDROE = standard deviation of earnings over the previous five years;

EL = earnings per share winsorized at 5 (-5); and

INVMR = inverse Mills ratio for endogenous auditor choice.

A total of 9,261 firm-year observations are used to calculate the descriptive statistics of the variables except *DISP*, for which 3,749 observations are used.

suggests that the average forecast dispersion is about 2.1 percent of the lagged stock price. *AUDITOR* has the mean (median) value of 0.890, indicating that 89 percent of the sample firm-year observations have hired a Big 5 auditor. The mean *ISPEC* is 0.042, suggesting that the average industry specialization percentage is about 4.2 percent. Firm size (*SIZE*), which is the logarithm of the mean market value of equity, is 5.486, equal to about \$2.4 million. The average earnings surprise (*SURPRISE*) is 0.152 and about 36 percent of the sample observations report loss (*LOSS*) for the year. The mean financial distress score (*ZMIJ*) is -1.765 and standard deviation of return on equity (*STDROE*) is 0.147. The mean forecast horizon (*HORIZON*) is 2.954, implying that the average number of calendar days between the forecast announcement date and subsequent actual earnings announcement date is 19. The mean number of analysts following (*NANA*) is 1.069, implying that an average of three analysts follow a firm included in our sample. The average earnings per share (*EL*) is 43 cents. These control variables have distributions similar to those reported by prior studies (see, e.g., Lang and Lundholm 1996; Eames and Glover 2003).

We perform both parametric t-tests and nonparametric Wilcoxon z-tests (both univariate) to detect any systematic difference in sample characteristics between the clients of Big 5 and non-Big 5. Untabulated results show that the clients of Big 5 tend to hire an industry specialist auditor (*ISPEC*, $t = 3.77$), tend to be larger firms (*SIZE*, $t = 42.15$), have shorter forecast horizons (*HORIZON*, $t = 8.46$), and have more analysts following the firm (*NANA*, $t = 3.98$). Further, Big 5 clients tend to have smaller earnings surprise (*SURPRISE*, $t = 15.50$), are less likely to incur loss (*LOSS*, $t = 8.44$), have less financial distress (*ZMIJ*, $t = 6.00$), have less volatile earnings (*STDROE*, $t = 12.62$), and greater earnings per share (*EL*, $t = 7.82$).

We also test whether firms that hire industry specialized auditors are different from others. We divide the full sample into three groups based on the degree of auditor industry specialization. We first rank all the observations based on the value of *ISPEC* and select the 25 percent of observations that have the highest value of *ISPEC*. These are the firm-year observations that hire industry expert auditors and we label them "expert auditors." Similarly, we select the 25 percent of observations that have the lowest value of *ISPEC*. These firms are those who hire non-industry experts and we label them "non-specialized auditors." The remaining 50 percent of the firms that do not belong to either group are labeled "other auditors."

We compare the characteristics of the observations that hire experts with those of observations that hire non-specialized auditors. The results reveal that firms hiring expert auditors are larger (*AUDITOR*, $t = 7.05$), record more frequent loss (*LOSS*, $t = 11.28$) and lower profitability (*STDROE*, $t = 3.48$), have longer forecast horizon (*HORIZON*, $t = 2.07$), experience less financial distress (*ZMIJ*, $t = 3.38$), and report less earnings surprise (*SURPRISE*, $t = 4.67$) and earnings level (*EL*, $t = 12.92$). Because of these differences, we control for these factors in our multivariate analyses to remove the possible influence of these factors on the dependent variable.

In Table 2, we report a correlation matrix, which contains Pearson product moment correlation between the regression variables. As expected, all of the variables are significantly correlated with forecast accuracy (*ACCY*). For example, *ACCY* is negatively correlated with *DISP* ($p < 0.01$) and positively correlated with *AUDITOR* ($p < 0.01$).¹⁵ The univariate correlation between *AUDITOR* and forecast dispersion (*DISP*) is -0.239, which is significant at 1 percent level. The univariate correlation between our alternative measure

¹⁵ All the significance levels reported in this study are based on two-tailed tests.

TABLE 2
Pearson Correlation Matrix

	ACCY	DISP	AUDITOR	ISPEC	SIZE	SURPRISE	LOSS	ZMIJ	HORIZON	NANA	STDROE	EL
DISP	-0.705***											
AUDITOR	0.196***	-0.239***										
ISPEC	0.044***	-0.072***	0.039***									
SIZE	0.285***	0.278***	0.401***	0.121***								
SURPRISE	-0.241***	0.186***	-0.158***	-0.036***	-0.276***							
LOSS	-0.139***	0.100***	-0.087***	0.054***	0.189***	-0.339***						
ZMIJ	-0.130***	-0.089***	-0.061***	0.010	0.213***	-0.123***	0.264***					
HORIZON	-0.224***	0.159***	-0.088***	-0.022**	0.083***	-0.194***	0.113***	0.068***				
NANA	0.070***	-0.020	0.041***	0.017	-0.044***	0.191***	-0.033***	-0.001	-0.104***			
STDROE	-0.224***	0.158***	-0.129***	-0.027***	0.580***	-0.248***	0.147***	0.209***	0.087***	-0.045***		
EL	0.108***	-0.087***	0.081***	-0.041***	-0.184***	0.347***	-0.499***	-0.366***	-0.110***	0.080***	-0.129***	
INVMR	0.038***	0.011	-0.062***	-0.105***	-0.095***	-0.027**	-0.931***	-0.234***	-0.045***	-0.038***	-0.061***	0.134***

***, ** Indicate significance at 1 percent and 5 percent levels in a two-tailed test, respectively.

Variable Definitions:

ACCY = accuracy in analysts' earnings forecasts, defined as the negative of the absolute difference between the forecast and actual earnings, scaled by price.

DISP = dispersion in analysts' earnings forecasts;

AUDITOR = dummy variable that takes the value of 1 (0) if the firm is affiliated with a Big 5 (non-Big 5) auditor;

ISPEC = industry specialist auditor measure, defined as the sum of the square root of the total assets of the clients of an auditor belongs to the industry of the client divided by total sum of the square root of the total assets of entire clients of the auditor;

SIZE = logarithm of market value of equity;

SURPRISE = this year's earnings minus last years' earnings deflated by stock price;

LOSS = coded as 0 (1) for firm-year observations with positive (negative) earnings;

ZMIJ = Zmijewski's financial distress score;

HORIZON = log of the average of the number of calendar days between mean forecast announcement date and subsequent actual earnings announcement date;

NANA = log of number of analysts following the client;

STDROE = standard deviation of earnings over the previous five years;

EL = earnings per share winsorized at 5 (-5); and

INVMR = inverse Mills ratio for endogenous auditor choice.

A total of 9,261 firm-year observations are used to calculate the correlation between the variables except the correlations with DISP for which 3,749 observations are used.

of audit quality, *ISPEC*, and *ACCY* is positive and significant at $p < 0.01$. The correlations among control variables are generally low, except the correlations between *AUDITOR* and *SIZE*, *EL* and *LOSS*, *STDROE* and *SURPRISE*, and *INVMR* and *LOSS*. The strong relationship between *AUDITOR* and *SIZE* is reasonable because large companies are more likely to hire Big 5 auditors. The relationship between *STDROE* and *SURPRISE* is also understandable because the stable companies are less likely to report greater surprise. Because earnings per share are used to derive the *LOSS* variable, the *EL* and *LOSS* variables are highly correlated. As reported in the bottom row of Table 2, the inverse Mills ratio (*INVMR*) is highly correlated with several firm characteristics.

Univariate Analyses

As a first stage, we perform univariate analyses to examine any association between auditor choice (auditor industry specialization) and analysts' forecast accuracy and dispersion. The results are reported in Table 3.

Panel A of Table 3 reports the mean (median) accuracy and dispersion of analysts' forecasts conditional on the auditor choice. The sample sizes (n) are reported in the first row. As reported in column (1), the mean (median) forecast accuracy is -0.0937 (-0.0137) when the auditor is one of the non-Big 5, whereas it is -0.0224 (-0.0024) when the auditor is one of the Big 5 (i.e., column (2)). The difference is negative and significant at 1 percent level in both t - and Wilcoxon z -tests ($t = -19.45$ and $z = -20.80$), suggesting that forecast accuracy is higher for the firms audited by a Big 5 auditor. Similarly, the forecast dispersion is lower for the firms audited by a Big 5 auditor as evidenced by the significant difference reported in the bottom two rows of Panel A of Table 3 ($t = 15.04$ and $z = 18.41$).

Panel B of Table 3 reports the results on the industry specialization. The "high *ISPEC*" observations that hire expert auditors (i.e., column (3)) have higher mean forecast accuracy (-0.0256 versus -0.0439) than the "low *ISPEC*" observations that hire non-specialized auditors (i.e., column (1)). The difference between the two is negative and significant at 1 percent level ($t = -5.51$ and $z = -4.58$).¹⁶ Similarly, the high *ISPEC* observations show lower forecast dispersion than the low *ISPEC* observations as reported in the bottom two rows of the Panel B of Table 3 ($t = 4.49$ and $z = 6.09$). In summary, all the univariate test results suggest that audit quality, measured by Big 5 and industry specialized auditor choice, is associated with the properties of analysts' earnings forecasts in the manner we hypothesize. However, because univariate tests are prone to the influence of other correlated omitted variables that can affect analysts' earnings forecast properties, we perform the following multivariate analyses.

Multivariate Analyses

We perform regressions using two analysts' forecast property variables, *ACCY* and *DISP*, as the dependent variables.

Audit Quality and Analysts' Forecast Accuracy

The accuracy regression results using Equation (1) are found in Table 4. Note that columns (1) and (4) do not include the *ISPEC* variable whereas the other models do. In

¹⁶ Panel B of Table 3 shows that the middle 50 percent observations (i.e., the "other auditors" category) have the mean (median) forecast accuracy of -0.0254 (-0.0025), which is similar to that of expert auditors (mean = -0.0256 and median = -0.0026 in column (3)). However, the difference is statistically insignificant ($t = 0.89$ and $z = 1.21$).

TABLE 3
Audit Quality
Univariate Comparisons of Forecast Accuracy and Dispersion

Panel A: Association between Auditor Choice and Forecast Accuracy and Dispersion

		(1) <i>AUDITOR</i> = 0 (<i>n</i> = 1,020)	(2) <i>AUDITOR</i> = 1 (<i>n</i> = 8,241)
Mean <i>ACCY</i> (median <i>ACCY</i>)		-0.0937 (-0.0137)	-0.0224 (-0.0024)
Difference [(1) - (2)]	t-test Wilcoxon test		-19.45*** -20.80***
		(1) <i>AUDITOR</i> = 0 (<i>n</i> = 385)	(2) <i>AUDITOR</i> = 1 (<i>n</i> = 3,364)
Mean <i>DISP</i> (median <i>DISP</i>)		0.0877 (0.0143)	0.0136 (0.0016)
Difference [(1) - (2)]	t-test Wilcoxon test		15.04*** 18.41***

Panel B: Association between Auditor Industry Specialization and Forecast Accuracy and Dispersion

		(1) <i>ISPEC</i> = Low 25% Non-Specialized (<i>n</i> = 2,320)	(2) <i>ISPEC</i> = Middle 50% Other Auditors (<i>n</i> = 4,626)	(3) <i>ISPEC</i> = High 25% Expert Auditors (<i>n</i> = 2,315)
Mean <i>ACCY</i> (median <i>ACCY</i>)		-0.0439 (-0.0034)	-0.0254 (-0.0025)	-0.0256 (-0.0026)
Difference [(1) - (3)]	t-test Wilcoxon test		-5.51*** -4.58***	
		(1) <i>ISPEC</i> = Low 25% Non-Specialized (<i>n</i> = 940)	(2) <i>ISPEC</i> = Middle 50% Other Auditors (<i>n</i> = 1,872)	(3) <i>ISPEC</i> = High 25% Expert Auditors (<i>n</i> = 937)
Mean <i>DISP</i> (median <i>DISP</i>)		0.0348 (0.0025)	0.0177 (0.0018)	0.0152 (0.0016)
Difference [(1) - (3)]	t-test Wilcoxon test		4.49*** 6.09***	

*** Indicate significance at 1 percent level in a two-tailed test.

Variable Definitions:

ACCY = accuracy in analysts' earnings forecasts, defined as the negative of the absolute difference between the forecast and actual earnings, scaled by price;

DISP = dispersion in analysts' earnings forecasts;

AUDITOR = a dummy variable that takes the value of 1 (0) if the firm is affiliated with a Big 5 (non-Big 5) auditor; and

ISPEC = industry specialist auditor measure, defined as the sum of the square root of the total assets of the clients of an auditor belonging to the industry of the client divided by total sum of the square root of the total assets of entire clients of the auditor.

TABLE 4
Multivariate Tests on the Association between Audit Quality and Forecast Accuracy

$$ACCY = \varphi_0 + \varphi_1 AUDITOR + \varphi_2 ISPEC + \varphi_3 (AUDITOR \times ISPEC) + \varphi_4 SIZE + \varphi_5 SURPRISE + \varphi_6 LOSS + \varphi_7 ZMIJ + \varphi_8 HORIZON$$

$$+ \varphi_9 STDROE + \varphi_{10} NANA + \varphi_{11} EL + \varphi_{12} INVMR + \text{year dummies} + \varepsilon$$

	Expected Sign	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	?	0.0188 (2.02)**	0.0177 (1.86)*	0.0130 (1.28)	-3.5398 (-1.14)	-3.4892 (-1.19)	-3.6459 (-1.18)
AUDITOR	+	0.0316 (4.95)***	0.0315 (4.93)***	0.0389 (5.03)***	0.0317 (4.97)***	0.0316 (5.22)***	0.0391 (5.07)***
ISPEC	+		0.0365 (1.05)	0.1407 (2.14)**		0.0354 (1.05)	0.1409 (2.14)**
AUDITOR $\times$ ISPEC	-			-0.1918 (-2.49)**			-0.1944 (-2.53)**
SIZE	+	0.0079 (11.43)***	0.0078 (11.38)***	0.0080 (11.44)***	0.0413 (1.42)	0.0407 (1.48)	0.0423 (1.46)
SURPRISE	-	-0.0224 (-3.22)***	-0.0223 (-3.22)***	-0.0221 (-3.17)***	-0.0220 (-3.18)***	-0.0220 (-3.15)***	-0.0218 (-3.14)***
LOSS	-	-0.0068 (-2.21)**	-0.0071 (-2.32)**	-0.0064 (-2.09)**	0.4390 (1.13)	0.4322 (1.18)	0.4519 (1.17)
ZMIJ	-	-0.0035 (-4.18)***	-0.0035 (-4.19)***	-0.0035 (-4.21)***	-0.0033 (-3.87)***	-0.0033 (-3.97)***	-0.0033 (-3.89)***
HORIZON	-	-0.0401 (-14.42)***	-0.0400 (-14.40)***	-0.0400 (-14.40)***	-0.0401 (-14.46)***	-0.0400 (-14.53)***	-0.0401 (-14.44)***

(continued on next page)

TABLE 4 (continued)

	Expected Sign	(1)	(2)	(3)	(4)	(5)	(6)
STDROE	-	-0.0193 (-2.81)***	-0.0193 (-2.81)***	-0.0197 (-2.85)***	-0.0194 (-2.82)***	-0.0194 (-2.97)***	-0.0197 (-2.87)***
NANA	+	0.0013 (1.10)	0.0013 (1.10)	0.0014 (1.18)	0.0012 (1.04)	0.0012 (1.00)	0.0013 (1.11)
EL	?	-0.0027 (-3.93)***	-0.0027 (-3.84)***	-0.0028 (-3.95)***	-0.0025 (-3.70)***	-0.0025 (-3.64)***	-0.0025 (-3.72)***
INVMR	?				4.2911 (1.14)	4.2287 (1.20)	4.4120 (1.18)
Year Dummies		Included	Included	Included	Included	Included	Included
($\varphi_1 + \varphi_3$) = 0				F = 1.96 (p = 0.162)			F = 2.16 (p = 0.142)
Adjusted R ₂		15.00%	15.00%	15.10%	15.03%	15.03%	15.13%
n		9,261	9,261	9,261	9,261	9,261	9,261

***, ** indicate significance at 1 percent, 5 percent, and 10 percent levels in a two-tailed test, respectively.

All the t-statistics are based on White's (1980) heteroscedasticity-corrected standard errors and clustering procedure by each firm. Year dummies are included but not reported.

Variable Definitions:

ACCY = accuracy in analysts' earnings forecasts;

AUDITOR = dummy variable that takes the value of 1 (0) if the firm is affiliated with a Big 5 (non-Big 5) auditor;

ISPEC = industry specialist auditor measure, defined as the sum of the square root of the total assets of the clients of an auditor in a specific industry divided by total sum of the square root of the total assets of entire clients of the auditor;

SIZE = logarithm of market value of equity;

SURPRISE = this year's earnings minus last years' earnings deflated by stock price;

LOSS = coded as 0 (1) for firm-year observations with positive (negative) earnings;

ZMIJ = Zmijewski's financial distress score;

HORIZON = log of the mean forecast horizon;

STDROE = standard deviation of earnings over the previous five years;

NANA = log of number of analysts following the client;

EL = earnings per share winsorized at 5 (-5); and

INVMR = inverse Mills ratio for endogenous auditor choice.

addition, columns (3) and (6) include the interaction between *AUDITOR* and *ISPEC* to see if the effect of *ISPEC* is different between Big 5 and non-Big 5 auditors. Finally, columns (4), (5), and (6) include the inverse Mills ratio (*INVMR*) for endogenous auditor choice as an additional control variable.

In Table 4, the coefficient on *AUDITOR* (ϕ_1) is positive and significant at $p < 0.01$ across different specifications, consistent with the prediction of H1 that Big 5 auditor choice is associated with higher forecast accuracy. For example, in column (2), the coefficient is 0.0315 with $t = 4.93$. The coefficient on our alternative proxy for audit quality (ϕ_2), *ISPEC*, is not significant in columns (2) and (5), but is positively significant at $p < 0.05$ in columns (3) and (6). For example, the coefficient in column (3) is 0.1407 with $t = 2.14$. The coefficient on the interaction between *AUDITOR* and *ISPEC* (ϕ_3) is negatively significant at $p < 0.05$ in both columns (3) and (6). Reported in the second row from the bottom of the table, the sum of the coefficients on *AUDITOR* and *AUDITOR* $\times$ *ISPEC* (i.e., $\phi_1 + \phi_3$) is not different from zero ($F = 1.96$ in column (3) and 2.16 in column (6)). Such insignificance implies that the impact of industry specialization on forecast accuracy is not significantly different from zero for the clients of the Big 5.¹⁷ However, the significant coefficient on *ISPEC* in columns (3) and (6) suggests that forecast accuracy is higher when a firm is audited by an industry specialist non-Big 5 auditor than when a firm is audited by a non-industry specialist non-Big 5 auditor.

The control variables have the predicted signs and are significant in most cases in columns (1), (2), and (3). Consistent with Lang and Lundholm (1996), the coefficient on *SIZE* is positively significant. The coefficients on *SURPRISE* and *HORIZON* are negatively significant throughout different specifications. As in Hwang et al. (1996), the coefficient on *LOSS* is negative in columns (1), (2), and (3). However, the coefficients on *SIZE* and *LOSS* turn insignificant in columns (4), (5), and (6) where we include the inverse mills ratio (*INVMR*), which is highly correlated with these two variables as reported in Table 2. The coefficients on *ZMIJ*, *STDROE*, and *EL* are always negatively significant, while the coefficient on *NANA* is always insignificant. However, the signs of the coefficients are in the expected positive direction.

To examine the economic significance of our findings, we place all the variables at their respective mean values except *AUDITOR* and *ISPEC* and calculate the expected accuracy of the forecasts. When we use the estimated coefficients in column (3), the clients of Big 5 auditors have an estimated forecast accuracy of -0.0383 , which means that the average difference between forecast and actual earnings is about 3.83 percent of the lagged stock price. For non-Big 5 auditors, the estimated accuracy is -0.0676 , which is about 76 percent $((0.0676 - 0.0383)/0.0383)$ lower than that of Big 5 auditors.

This accuracy difference is also observed across industry specialist and non-specialist auditors, although the difference between Big 5 clients and non-Big 5 clients is smaller for specialist auditors. For example, if the value of *ISPEC* is 0.1 (i.e., when the proportion of a client's sum of square root of total assets in a particular industry is 10 percent of all clients' sum of square root of total assets), the estimated average forecast accuracy for a Big 5 client is -0.0409 , while that for non-Big 5 clients is -0.0606 , holding other factors

¹⁷ We also test if the audit quality is related to the accuracy of the year $t+1$ and $t+2$'s forecasts. For both tests, the coefficient of *AUDITOR* is strongly significant ($\phi_1 = 0.0513$ with $t = 3.65$ when 6,713 observations of $t+1$'s earnings forecasts are used, and $\phi_1 = 0.0738$ with $t = 3.64$ when 3,088 observations of $t+2$'s earnings forecasts are used). These results suggest that the audit choice matters persistently for long-horizon. The coefficients of *ISPEC* and the interaction term between *AUDITOR* and *ISPEC* are not significant in both regressions.

constant (i.e., when we set all variables other than *AUDITOR* at the mean value).¹⁸ The difference is about 48 percent, which is much smaller than the 76 percent difference reported in the Big 5/non-Big 5 situation above. This smaller difference in specialist auditors can be attributed mostly to the increased forecast accuracy for non-Big 5 clients (-0.0676 versus -0.0606), while the clients of Big 5 auditors show smaller change (-0.0383 versus -0.0409). In contrast, the difference is larger if the auditor is not an industry specialist. If we set *ISPEC* equal to 0.01, the difference is 102 percent (-0.0363 versus -0.0733).

Audit Quality and Analysts' Forecast Dispersion

The forecast dispersion regression results using Equation (3) are reported in Table 5. To calculate forecast dispersion (*DISP*), we require that at least four analysts' forecasts are available for our estimation period (i.e., about two months). Thus, the sample size decreases to 3,749. Similar to Table 4, columns (1) and (4) in Table 5 do not include *ISPEC* or the interactions between *ISPEC* and *AUDITOR*, whereas columns (3) and (6) include both of them. Columns (2) and (5) include *ISPEC*, but not the interaction. In addition, columns (4), (5), and (6) include the inverse Mills ratio (*INVMR*), but columns (1), (2), and (3) do not.

Across all the specifications in Table 5, *AUDITOR* has a negative and significant coefficient. The coefficient on *ISPEC* is also significant and negative. However, the coefficient on the interaction between *AUDITOR* and *ISPEC* is positive and significant at $p < 0.05$. The sum of the coefficients on *AUDITOR* and *AUDITOR* $\times$ *ISPEC* ($\phi_1 + \phi_3$) is not significantly different from zero ($F = 0.01$ in column (3) and $F = 0.00$ in column (6) as reported in the second to the bottom row of the Table 5). In general, these results suggest that the analysts' forecast dispersion for the clients of Big 5 auditors is lower than for the clients of non-Big 5 auditors. In addition, the forecast dispersion for the clients of the industry specialist non-Big 5 auditors is lower than that of the clients of non-industry specialist non-Big 5 auditors. However, clients of Big 5 auditors show no difference, regardless of whether the auditors are industry specialists.¹⁹

Other than these results, *DISP* tends to increase with the magnitude of earnings surprise (*SURPRISE*) and the forecast horizon (*HORIZON*). Consistent with Lang and Lundholm (1996), *DISP* is negatively correlated with firm size (*SIZE*). *STDROE* and *ZMIJ* have statistically significant univariate correlations with *DISP* as reported in Table 2, but they are insignificant once *AUDITOR* and other control variables are included in the regression model. The explanatory power of the models ranges from 11.50 to 11.78 percent.²⁰

¹⁸ There are ten non-Big 5 auditors that have an *ISPEC* value greater than or equal to 0.10 in 21 industries. For example, our methodology classifies BDO Seidman as a machinery and computer equipment industry (SIC code 35) expert and Grant Thornton as an expert in the industries of electronic and other electrical equipment and components (SIC code 36) and wholesale trade-durable goods (SIC code 50).

¹⁹ When we perform additional tests using the dispersion of the forecasts for year $t+1$ ($n = 3,739$), we find consistent results as reported in Table 5. For example, the coefficient on *AUDITOR* is -0.0605 ($t = -4.79$), that on *ISPEC* is -0.1998 ($t = -4.01$), and that on the interaction term is 0.1958 ($t = 2.91$). However, when we use the forecasts for year $t+2$ ($n = 963$), we fail to find any significant relations with respect to the variables of interest, possibly due to the lack of statistical power caused by small sample size.

²⁰ The results reported in column (3) of Table 5 suggest that forecast dispersion is around 2.30 percent (6.32 percent) of lagged stock price for the clients of Big 5 (non-Big 5) auditors if we set all other variables at their respective mean value. This suggests that analysts' forecast dispersions for non-Big 5 clients are approximately 1.7 times greater than those of Big 5 clients. In addition, when we set all the values except *ISPEC* at their respective mean, the forecast dispersion is around 2.29 percent (5.26 percent) of lagged stock price for the clients of Big 5 (non-Big 5) industry specialist auditors (i.e., *ISPEC* = 0.1), whereas it is 2.32 percent (7.17 percent) for the clients of Big 5 (non-Big 5) non-industry specialist auditors (i.e., *ISPEC* = 0.01). These results also confirm the findings in Table 5 that industry specialization is associated with audit quality for non-Big 5 auditors but not for Big 5 auditors.

TABLE 5
Multivariate Tests on the Association between Audit Quality and Forecast Dispersion
 $DISP = \varphi_0 + \varphi_1 AUDITOR + \varphi_2 ISPEC + \varphi_3 (AUDITOR \times ISPEC) + \varphi_4 SIZE + \varphi_5 SURPRISE + \varphi_6 ZMIJ + \varphi_7 HORIZON$
 $+ \varphi_8 STDROE + \varphi_9 INVMR + year\ dummies + \varepsilon$

	Expected Sign	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	?	0.0175 (0.77)	0.0208 (0.90)	0.0246 (1.05)	0.0034 (0.10)	0.0129 (0.36)	0.0103 (0.29)
AUDITOR	-	-0.0425 (-4.17)***	-0.0426 (-4.19)***	-0.0506 (-4.06)***	-0.0424 (-4.16)***	-0.0425 (-4.18)***	-0.0509 (-4.06)***
ISPEC	-		-0.0905 (-2.03)**	-0.2114 (-2.22)**		-0.0891 (-1.99)**	-0.2113 (-2.22)**
AUDITOR × ISPEC	+			0.2083 (2.02)**			0.2126 (2.04)**
SIZE	-	-0.0059 (-7.44)***	-0.0057 (-7.36)***	-0.0058 (-7.50)***	-0.0059 (-7.49)***	-0.0057 (-7.40)***	-0.0058 (-7.53)***
SURPRISE	+	0.0158 (2.26)**	0.0157 (2.26)**	0.0156 (2.25)**	0.0159 (2.29)**	0.0158 (2.28)**	0.0158 (2.28)**
ZMIJ	+	0.0017 (1.43)	0.0017 (1.48)	0.0017 (1.44)	0.0018 (1.59)	0.0018 (1.59)	0.0018 (1.60)

(continued on next page)

TABLE 5 (continued)

	Expected Sign	(1)	(2)	(3)	(4)	(5)	(6)
<i>HORIZON</i>	+	0.0260 (3.86)***	0.0260 (3.85)***	0.0262 (3.90)***	0.0261 (3.86)***	0.0260 (3.84)***	0.0264 (3.90)***
<i>STDROE</i>	+	0.0106 (1.24)	0.0104 (1.22)	0.0104 (1.23)	0.0105 (1.24)	0.0104 (1.22)	0.0104 (1.22)
<i>INVMR</i>	?				0.0182 (0.55)	0.0100 (0.30)	0.0183 (0.55)
<i>Year Dummies</i> ($\varphi_1 + \varphi_3$) = 0		Included	Included	Included	Included	Included	Included
Adjusted R ²		11.50%	11.62%	11.78% (p = 0.931)	11.50%	11.62%	11.78% (p = 0.970)
n		3,749	3,749	3,749	3,749	3,749	3,749

***, **, * Indicate significance at 1 percent, 5 percent, and 10 percent levels in a two-tailed test, respectively.

All the t-statistics are based on White's (1980) heteroscedasticity-corrected standard errors and clustering procedure by each firm. Year dummies are included but not reported.

Variable Definitions:

DISP = dispersion in analysts' earnings forecasts;

AUDITOR = dummy variable that takes the value of 1 (0) if the firm is affiliated with a Big 5 (non-Big 5) auditor;

ISPEC = industry specialist auditor measure, defined as the sum of the square root of the total assets of the clients of an auditor in a specific industry divided by total sum of the square root of the total assets of entire clients of the auditor;

SIZE = logarithm of market value of equity;

SURPRISE = this year's earnings minus last years' earnings deflated by stock price;

ZMIJ = Zmijewski's financial distress score;

HORIZON = log of mean forecast horizon;

STDROE = standard deviation of earnings over the previous five years; and

INVMR = inverse Mills ratio for endogenous auditor choice.

Sensitivity Analyses

Although they are not separately tabulated, we perform various sensitivity analyses to check the robustness of our findings. First, we repeat the tests using a sample of firms that do not change the auditor in year $t-1$ or $t+1$ (i.e., firms that hire the current auditor in the year before and after the event year). We perform this test because auditor change could be related to the change of the quality of audit service, but our main results are not affected. Second, we look at a subsample of firms that receive a clean audit opinion in the current year. Our supposition is that the likelihood of potential audit quality impairment is higher among these firms than firms that receive a non-clean opinion, but empirical results are the same as before.

Third, we note that our sample period includes the demise of Arthur Andersen. Thus, we compare the forecast accuracy of firms that are audited by Arthur Andersen and non-Big 5 auditors. The result supports the view that Arthur Andersen generally provides higher quality audit service than other non-Big 5 auditors.²¹

Fourth, we perform tests separately for: (1) loss-reporting versus profit-reporting clients, and (2) clients followed by at least three analysts versus those followed by fewer than three analysts,²² and (3) large clients (i.e., firms of greater than median size) versus small clients. We find consistent results in all cases.

Fifth, we estimate regressions year-by-year to examine if our main finding is a year-specific phenomenon.²³ Additionally, we perform Fama-MacBeth type regressions. All the results generally support our main findings in Tables 4 and 5.

Finally, to further examine the effect of auditor expertise (i.e., industry specialization) on audit quality, we run the following Equation (5) separately for Big 5 and non-Big 5 client samples. We perform this analysis because the previously reported univariate results reveal that the clients of Big 5 and non-Big 5 might differ on several dimensions:

$$\begin{aligned} ACCY = & \varphi_0 + \varphi_1 ISPEC + \varphi_2 SIZE + \varphi_3 SURPRISE + \varphi_4 LOSS + \varphi_5 ZMIJ \\ & + \varphi_6 HORIZON + \varphi_7 STDROE + \varphi_8 NANA + \varphi_9 EL \\ & + year\ dummies + \varepsilon. \end{aligned} \quad (5)$$

Unlike Equation (1), Equation (5) allows the regression coefficients to vary across Big 5 and non-Big 5 clients. The empirical results are consistent as before; the coefficient on *ISPEC* is not significant for Big 5 clients ($\varphi_1 = 0.0164$ with $t = 0.72$), while it is positively significant for non-Big 5 clients ($\varphi_1 = 0.1367$ with $t = 2.54$).²⁴ When we estimate a

²¹ The coefficients on both *AUDITOR* and *ISPEC* remain positive and significant when we compare the clients of other Big 5 auditors with those of non-Big 5 auditors. However, both *AUDITOR* and *ISPEC* are not statistically different from zero when we compare Arthur Andersen's clients with the other Big 5's clients.

²² For this second test, we use the Equation (1) only because the forecast dispersion is defined only when the number of analysts following the firm is at least four.

²³ For the regressions on forecast accuracy, except for year 2001's results, the coefficient on *AUDITOR* has the positive sign in all years and it is statistically significant at the conventional level in the other years. The coefficient on *ISPEC* is negative but insignificant in years 1996 and 2001. However, the coefficients of the other years are positive and three of them are significant. The interaction terms between *AUDITOR* and *ISPEC* are always negative and three of them are significant. Overall, these results are consistent with those reported in Table 4. For the regressions on forecast dispersion, except for year 2000's results, the coefficients on *AUDITOR* are negative in all years and significant in all but one year. The coefficients on *ISPEC* have negative signs in all years except for year 2000 and two of them are significant. These results are also largely consistent with those reported in Table 5.

²⁴ When we add the inverse Mills ratio (*INVMR*) as an additional control variable to this equation, the empirical results are almost identical.

modified version of Equation (3) using *DISP* as the dependent variable, we also find similar results; *ISPEC* is negatively significant ($\varphi_1 = -0.1601$ with $t = -1.82$) only in the non-Big 5 sample. Once again, these results suggest that industry expertise matters only to non-Big 5 auditors.

We perform yet another test to examine the impact of auditor size on forecast accuracy using the following Equation (6):

$$\begin{aligned} ACCY = & \varphi_0 + \varphi_1 AUDITOR + \varphi_2 SIZE + \varphi_3 SURPRISE + \varphi_4 LOSS + \varphi_5 ZMIJ \\ & + \varphi_6 HORIZON + \varphi_7 STDROE + \varphi_8 NANA + \varphi_9 EL \\ & + year\ dummies + \epsilon. \end{aligned} \quad (6)$$

We estimate this model separately in industry expert and non-specialized auditor subsamples. The coefficient on *AUDITOR* is significantly positive in both subsamples, although it is greater in the non-specialized auditor subsample. When we use *DISP* as the dependent variable, the coefficient on *AUDITOR* is negatively significant in both regressions but the magnitude is larger in the non-specialized auditor sample.²⁵

Further Tests on the Potential Endogeneity of Auditor Choice

The next sensitivity analysis relates to the endogeneity of the auditor choice by clients. Factors that determine the choice of Big 5 or non-Big 5 auditors are likely to be also related to the properties of analysts' earnings forecasts. To alleviate this problem, we include the inverse Mills ratio for Big 5 auditor choice in our main analyses. While this approach is widely used in the accounting literature, Larcker and Rusticus (2007) doubt the appropriateness of two-stage regressions approaches. Specifically, they claim that: (1) it is incredibly difficult to find instruments for the first stage that are exogenous to the second stage and (2) often the first stage models do not have a "reasonable fit."

Consequently, we perform two more tests to examine whether our results are driven by endogenous auditor choice. First, we compare analysts' forecast properties in samples of firms that change the auditor from year $t-1$ to t . We then categorize the auditor-changing firms in a 2×2 matrix based on the former and the new auditor type (i.e., whether a Big 5 or not). Subsequently, we compare the forecast properties in each cell.

Untabulated results show that the audit quality might deteriorate rather quickly after the auditor changes from a Big 5 to non-Big 5, while there may not be a substantial improvement in audit quality immediately after changing the auditor from a non-Big 5 to a Big 5 (although we do observe an insignificant increase in forecast accuracy after the change). The clients who change auditor from a Big 5 to another Big 5 auditor or from a non-Big 5 to another non-Big 5 do not experience significant accuracy changes.²⁶ Thus, in a subsample of auditor-changing firms, we confirm our previous claim that our findings are unlikely to be driven by the endogenous auditor choice.

We also compare the accuracy of the forecasts at year t for four different cells in a 2×2 matrix. We find that the accuracy at year t is the highest for the firms that change

²⁵ In the industry expert auditor subsample, the coefficient on *AUDITOR* is significant at the 10 percent level for both *ACCY* and *DISP* regressions. Here, the statistical significance is lower presumably due to the lack of statistical power. We observe that most industry specialist auditors are affiliated with a Big 5 firm. In our sample, approximately 93 percent of industry expert auditors come from a Big 5 firm, whereas only about 76 percent of non-specialized auditors belong to a Big 5 firm.

²⁶ We also check the forecast dispersion changes surrounding auditor changes. We find that the firms changing auditors from non-Big 5 to Big 5 show marginal decrease of forecast dispersion ($t = 1.71$), while the other three categories of auditor changes show no significant differences.

auditor from a Big 5 to another Big 5, followed by the firms that change auditor from a non-Big 5 to a Big 5. The firms that change auditor from a Big 5 to a non-Big 5 or from a non-Big 5 to another non-Big 5 show insignificant difference from each other and have the lowest accuracy among the four cells.²⁷ These results also support the proposition that the higher audit quality provided by the Big 5 is related to the properties of analysts' forecasts.

V. CONCLUDING REMARKS

According to the Statement of Financial Accounting Concepts No. 1 (FASB 1978), an objective of financial reporting is to provide information useful to present and potential investors, creditors, and other users in making rational investment, credit, and similar decisions. An independent audit renders reliability to the financial statement numbers by reviewing financial statements and other information, enhancing their credibility to providers and their decision-usefulness to users (FASB 1978).

Because financial reporting reliability is unobservable, prior studies have largely relied on stock price to infer how market participants evaluate the quality of an audit (e.g., Teoh and Wong 1993; Krishnan 2003; Balsam et al. 2003; Khurana and Raman 2004). In this study, we take an alternative approach, examining whether perceived audit quality relates to the usefulness of financial statements by focusing on the properties of earnings forecasts issued by those analysts. Our empirical evidence using auditor size and industry specialization as proxies for higher audit quality is generally consistent with our predictions. We find a positive relation between audit quality and forecast accuracy and a negative relation between audit quality and forecast dispersion.

In closing, we mention some potential caveats of the study. First, while suggestive of the link, our study does not show a causal relationship between audit quality and analyst forecast properties. Instead, we rely on association tests to document the relation. Proving causality would require knowledge of whether and how individual users (i.e., security analysts) improve their decisions owing to high-quality audits. Unfortunately, we did not have access to such information. Second, the documented association between audit quality and analyst forecast properties might not necessarily be due to increased financial reporting reliability. We are not able to rule out the possibility of some unknown omitted factors influencing our empirical results. Despite these factors, this study takes an early step in understanding of how audit quality relates to financial statement users' decision-making processes.

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²⁷ In terms of forecast dispersion, we find that the dispersion is lowest for the firms that change auditors from Big 5 to another Big 5, followed by the firms that change auditors from non-Big 5 to Big 5. However, the difference between the two is not significant. The firms that change auditors from non-Big 5 to another non-Big 5 or Big 5 to non-Big 5 show the highest dispersion and the magnitude of the dispersion is significantly greater than that of the former two change categories. In summary, the results on forecast dispersion also support the findings on forecast accuracy that Big 5 auditor choice is associated with better forecasting performance by analysts.

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The Effect of Firms' Depreciation Method Choice on Managers' Capital Investment Decisions

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ABSTRACT: This study examines whether straight-line depreciation, relative to accelerated depreciation, causes non-executive managers to make non-value-maximizing capital investment decisions. To do this, I conduct experiments in which managers must decide whether to continue using an existing asset or invest in a replacement asset. By design, replacing the existing asset yields higher cash flows and managers are aware of this fact. However, if the asset is replaced, then the greater remaining book value under straight-line depreciation relative to accelerated depreciation causes earnings to be lower. Lower earnings and psychological forces may push managers of firms that use straight-line depreciation away from making the economically efficient capital investment decision. The results suggest that managers of firms that use straight-line depreciation are less likely to invest in a replacement asset than are managers of firms that use accelerated depreciation. Further, the results suggest that managers perceive that an asset depreciated using straight-line depreciation has provided less retrospective utility than an asset depreciated using accelerated depreciation. In turn, I find that depreciation method-induced differences in managers' retrospective utility perceptions influence their prospective utility perceptions, which, in turn, influence managers' asset replacement decisions. By theoretically and empirically linking firms' depreciation method choice to managers' capital investment decisions, I provide evidence that a seemingly innocuous choice made for external financial reporting purposes can cause managers to make non-value-maximizing capital investment decisions.

Keywords: *accounting depreciation; accounting method choice; accounting book value; capital investment; mental accounting; mental depreciation.*

I thank Wendy Bailey, Mark Cecchini, John Core (editor), Tim Douppnik, Sukari Farrington, Louis Fayard, Rick Hatfield, Noah Jackson, Kelvin Liu, Terence Pitre, Subhash Sharma, Brad Tuttle, Scott Vandervelde, Rich White, YiJing Wu, two anonymous reviewers, and workshop participants at the University of South Carolina for providing constructive comments. Louis Fayard and Kelvin Liu provided many helpful suggestions during the formation of this study.

Editor's note: This paper was accepted by John Core.

*Submitted May 2006
Accepted August 2007*

I. INTRODUCTION

The United States Census Bureau reports that businesses spent an aggregate of approximately \$1.05 trillion on capital investments in 2004, or an average of \$17.2 million per firm.¹ Publicly traded firms in North America spent an aggregate of approximately \$730 billion on capital investments in 2004, or an average of \$102.4 million per firm.² Given the magnitude of annual capital expenditures, efforts to understand the factors that influence managers' capital investment decisions are of considerable practical importance. Not only do capital investment decisions impact firms' market value (McConnell and Muscarella 1985) and their long-term survival (Klammer et al. 1991), but they also impact the economic well being of society as a whole (Harris and Raviv 1996).

There is a considerable amount of academic and professional literature that examines how firms (1) raise external funds to finance capital investments (e.g., Geddes 2003; Jenkinson and Ljungqvist 2001; Tinic 1988) and (2) choose from among competing capital investment options (e.g., Graham and Harvey 2001; Klammer and Walker 1984; Schall et al. 1978). However, prior research has not examined whether or why firms' depreciation method choice influences managers' capital investment decisions. Concern about this issue arises, in part, because academics have repeatedly speculated that the accounting treatment of historical costs may make it psychologically difficult for managers to regard them as irrelevant (Horngren et al. 2005; Morse et al. 2003; Titard 1993).

This study examines whether straight-line depreciation, relative to accelerated depreciation, causes non-executive managers to make non-value-maximizing capital investment decisions.³ To do this, I conduct three experiments in which managers must decide whether to continue using an existing asset or invest in a replacement asset. By design, replacing the existing asset yields higher cash flows and managers are aware of this fact. However, if the asset is replaced, then the greater remaining book value under straight-line depreciation relative to accelerated depreciation causes earnings to be lower. At the same time, managers are informed that they receive no earnings-based compensation, which should direct their attention toward the cash flow consequences of their decision. However, even in the absence of earnings-based compensation, theory suggests that earnings considerations and psychological forces may push managers of firms that use straight-line depreciation away from making the economically efficient capital investment decision.

While prior research has examined the effect of earnings considerations on the capital investment decisions of executive managers who have earning-based compensation (e.g., Dechow and Sloan 1991; Graham et al. 2005), I examine the effect of earnings considerations on the capital investment decisions of non-executive managers who have no earnings-based compensation. Even in the absence of earnings-based compensation, earnings considerations are likely to influence the decisions of non-executive managers because (1) the goals emphasized by executive managers (e.g., achieving earnings targets and thresholds) are likely to influence the goals emphasized by non-executive managers (Harrell 1977) and (2) the culture in many organizations places a significant emphasis on achieving earnings targets and thresholds (Jensen 2001; Morgan 1997).

¹ See the *Annual Capital Expenditures* survey published by the U.S. Census Bureau (2005).

² Capital expenditure data is obtained from Compustat for all available firms in Canada, Mexico, and the United States.

³ Bowen et al. (1995) report that 68.7 percent of firms use the straight-line method only, 25.1 percent use a combination of the straight-line method and an accelerated method, and 4.9 percent use an accelerated method only.

The experimental results reveal that non-executive managers of firms that use straight-line depreciation are significantly less likely to invest in a replacement asset than are non-executive managers of firms that use accelerated depreciation despite the fact that the economically efficient decision is to invest in the replacement asset. This finding holds regardless of whether (1) one depreciation method results in a gain on disposal and the other depreciation method results in a loss on disposal, (2) both depreciation methods result in gains on disposal of differing magnitudes, or (3) both depreciation methods result in losses on disposal of differing magnitudes.

Firms' depreciation method choice may also have an indirect influence on managers' capital investment decisions if managers apply the mental accounting processes they use for consumer durable goods to situations involving business assets. In consumer situations, the more that a durable good has been used, the greater the amount of mental depreciation and the lower the mental book value (Heath and Fennema 1996; Okada 2001). Because of the cause-and-effect relation between past physical usage of a consumer durable good and the amount of its mental book value, managers may subconsciously view a business asset's accounting book value as being informative about the asset's retrospective utility and/or prospective utility. The experimental results reveal that non-executive managers perceive that an asset depreciated using straight-line depreciation has provided less retrospective utility than an asset depreciated using accelerated depreciation. In turn, I find that depreciation method-induced differences in managers' retrospective utility perceptions influence their prospective utility perceptions, which, in turn, influence managers' asset replacement decisions.

An important feature of the experiments is that they provide participants with ample information about the capital asset being considered for replacement (i.e., physical condition, original useful life, remaining useful life, current age, and market value). In the presence of this information, a firm's depreciation method choice and/or the asset's accounting book value are uninformative about the asset. Not only do all participants receive identical information about the asset being considered for replacement, but participants are also able to recall that information with equal accuracy across the experimental conditions. Further, the vast majority of participants understand that the firm's depreciation method choice does not correspond to the deterioration profile of the asset being considered for replacement, and excluding the participants who hold the opposite view does not alter any results. Thus, the conclusions of this study are robust to a variety of alternative explanations.

This study extends prior accounting research in three interrelated ways. First, this study provides initial evidence that firms' depreciation method choice influences non-executive managers' capital investment decisions. Prior research suggests that capital investment decisions are influenced by economic considerations (Klammer et al. 1991; Schall et al. 1978), affective reactions (Kida et al. 2001), decision-making domains (Moreno et al. 2002), choice difficulty (Sawers 2005), prior investment decisions (Arkes and Blumer 1985; Staw 1976), and earnings considerations (Bhojraj and Libby 2005; Bushee 1998; Darrough and Rangan 2005; Dechow and Sloan 1991; Graham et al. 2005; Shehata 1991). However, research has not explored the relation between firms' depreciation method choice and managers' capital investment decisions despite the fact that (1) accounting method choices influence the informational outputs from accounting systems and (2) these informational outputs are common inputs into managers' capital investment decisions (Hilton 2002; Horngren et al. 2005).

Second, this study provides initial evidence about *why* (not just whether) depreciation method-induced variations in accounting information influence the judgments of decision makers. Prior research has examined the effect of cosmetic variations in accounting information on the judgments of investors (Beaver and Dukes 1973; Chen and Schoderbek 2000;

Hand 1990), non-professional investors (Maines and McDaniel 2000), analysts (Hirst and Hopkins 1998; Hopkins et al. 2000), and buyers and sellers in laboratory markets (Buchheit 2004; Kachelmeier 1996). Despite a large body of evidence on the potential effects of cosmetic variations in accounting information, researchers have paid limited attention to the psychological consequences that different accounting methods engender. This study extends prior research by theoretically and empirically linking depreciation method-induced variations in managers' prospective utility perceptions to their capital investment decisions.

Third, the results of this study extend accounting choice research by providing evidence that firms' depreciation method choice has indirect economic consequences even in the *absence* of contracting considerations.⁴ The prevailing view in the accounting literature is that firms' choice between different depreciation methods has indirect economic consequences only in the *presence* of contracting considerations (Fields et al. 2001; Holthausen and Leftwich 1983; Leftwich 1981; Ricks 1982; Watts and Zimmerman 1986). By providing evidence that the array of consequences associated with different depreciation methods may be broader than previously thought, this study challenges the view that "depreciation is one accounting issue where the effects of the different methods are obvious and well understood" (Ricks 1982, 71).

The remainder of this paper is organized as follows. Section II formulates the hypotheses. Sections III and IV describe the experiments and discuss the results. Section V discusses and evaluates a variety of alternative explanations for the experimental findings. The final section summarizes the results and discusses certain limitations.

II. HYPOTHESES DEVELOPMENT

Earnings Considerations and Managerial Decision Making

Evidence suggests that firms increasingly use sophisticated capital budgeting techniques (Klammer et al. 1991; Schall et al. 1978), although those techniques are not always the sole basis for choosing among competing capital investment options (Klammer and Walker 1984). Graham et al. (2005) report that (1) almost half of the CFOs in their survey would forgo a positive net present value project if it caused earnings to fall below the consensus forecast and (2) CFOs feel that they are making appropriate decisions when they forgo economic value in order to deliver smooth earnings or meet an earnings target. Bhojraj and Libby (2005) find that managers are more likely to choose projects that maximize short-term earnings than projects that maximize total cash flows in response to increased capital market pressure.

Archival research also suggests an association between earnings considerations and managers' capital investment decisions. For example, Bushee (1998) and Darrrough and Rangan (2005) report evidence suggesting that managers cut R&D expenditures to meet earnings targets, and Dechow and Sloan (1991) find that as CEOs approach their final years in office they spend less on R&D to improve their firm's short-term earnings performance.⁵ Shehata (1991) finds that the requirement to immediately expense R&D caused firms that formerly capitalized such expenditures to reduce their current R&D investments.

The above studies focus on the decisions of executive managers who have earnings-based compensation. However, many significant business decisions are made by non-executive managers who have no earnings-based compensation. There are at least two

⁴ Indirect economic consequences arise because earnings are used as contractual arguments in, for example, debt covenants (Watts and Zimmerman 1986), executive bonus contracts (Healy 1985; Holthausen et al. 1995), and profit sharing arrangements (Jackson et al. 2004).

⁵ However, Murphy and Zimmerman (1993) conclude that reductions in R&D expenditures around CEO departures are mostly due to poor firm performance.

reasons why non-executive managers who have no earnings-based compensation will pay attention to the earnings-related consequences of their decisions. First, non-executive managers are likely to be aware of the metrics used to evaluate executive managers (e.g., earnings), and that knowledge is likely to influence the performance metrics that non-executive managers seek to maximize. Ittner et al. (2003) and Hopwood (1974) report evidence that managers evaluate their subordinates using the same criteria that are used to evaluate them, and Harrell (1977) finds that the goals emphasized by higher-level managers influence the goals emphasized by lower-level managers.

Second, the culture in many organizations places a significant emphasis on achieving earnings targets and thresholds. Jensen (2001) contends that corporate budgeting has created a culture within organizations that focuses managers' attention on earnings-related considerations. Further, Morgan (1997) suggests that certain groups within organizations, such as accountants, influence organizational culture by emphasizing the primacy of financial measures. Hence, prior literature suggests that non-executive managers are likely to pay attention to the earnings-related consequences of their decisions even when they receive no earnings-based compensation. As discussed in Section III, the experimental materials inform participants that they receive no earnings-based compensation.

If earnings considerations influence managers' capital investment decisions, then managers may be more reluctant to replace an existing capital asset that has a high accounting book value than an existing capital asset that has a low accounting book value. At any point during the life of a capital asset, straight-line depreciation results in a larger accounting book value than accelerated depreciation. As a result, straight-line depreciation results in a larger financial statement loss (or a smaller financial statement gain) upon asset disposal/replacement than accelerated depreciation. I propose that depreciation method-induced differences in the magnitude of an asset's accounting book value influence managers' willingness to invest in a new capital asset. Hypothesis 1, stated in alternative form, is as follows:

H1: Managers are less likely to invest in a new capital asset when the asset being replaced has been depreciated using straight-line depreciation than when the asset being replaced has been depreciated using accelerated depreciation.

As discussed in Section III, the experiments are designed such that the economically efficient capital investment decision is to invest in a new capital asset, so participants who do not invest forgo real economic value.

Mental Accounting, Mental Depreciation, and Perceived Utility

Individuals maintain mental accounting systems that allow them to mentally track the costs and benefits of transactions and events over time (Gourville and Soman 1998; Prelec and Loewenstein 1998; Thaler 1999). According to Thaler (1999, 183), "mental accounting is the set of cognitive operations used by individuals and households to organize, evaluate, and keep track of financial activities." Henderson and Peterson (1992, 95) observe that "a mental account is simply a category that includes the advantages and disadvantages (positively and negatively evaluated information) of the event or option (element) being categorized." The grouping of related activities and events within mental accounts or categories is spontaneous and may occur with minimal effort (Kahneman and Tversky 1984).

When the cost of a consumer transaction precedes its benefits (e.g., the purchase of a long-lived durable good), consumers may engage in a cognitive process known as "mental depreciation" (Heath and Fennema 1996). This process involves allocating (or mentally

depreciating) costs over time and use to the durable good's mental account in order to mentally align costs and benefits. At the same time, consumers post the hedonic benefits of consuming the durable good to its mental account, which, together with mental depreciation, temporally aligns the benefits and costs of owning the durable good. At any point in time during the durable good's life, the portion of cost that has not been allocated to the durable good's mental account comprises its "mental book value" (Okada 2001), which is the consumer analog of an asset's accounting book value.

If individuals apply the mental accounting processes that they use for consumer durable goods to settings involving business assets, then firms' depreciation method choice may influence managers' capital investment decisions. In consumer situations, the more that a durable good has been used, the greater the amount of mental depreciation and the lower the mental book value (Heath and Fennema 1996; Okada 2001). Because of the cause-and-effect relation between past physical usage of a consumer durable good and the amount of its mental book value (i.e., low physical usage implies a high mental book value, while high physical usage implies a low mental book value), managers may subconsciously view a business asset's accounting book value as an indicator of the *retrospective* (past) utility that the asset has provided. If this perspective is valid, then managers will perceive that an asset depreciated using straight-line depreciation (i.e., an asset having a high accounting book value) has provided less retrospective utility than an asset depreciated using accelerated depreciation (i.e., an asset having a low accounting book value). Hypothesis 2, stated in alternative form, is as follows:

H2: Managers perceive that an asset depreciated using straight-line depreciation has provided less *retrospective* utility than an asset depreciated using accelerated depreciation.

By their nature, durable goods have definite rather than indefinite lives. The more of a durable good that has been consumed in the past, the less of it there is to consume in the future. For example, a given piece of machinery may be capable of 4,000 hours of usage before it becomes uneconomical to use. If 3,000 hours have been used in the past, then only 1,000 hours can be used in the future. As a result, managers' perceptions about an asset's retrospective utility should be inversely related to their perceptions about an asset's *prospective* (future) utility. Hypothesis 3, stated in alternative form, is as follows:

H3: Managers' perceptions about an asset's *retrospective* utility are inversely related to their perceptions about an asset's *prospective* utility.

Perceived Utility and Capital Investment

The prescription that managers should make capital investment decisions based upon the incremental costs and benefits associated with alternative courses of action is noncontroversial (Hilton 2002; Horngren et al. 2005). However, certain aspects of capital investment decisions are inherently subjective, which suggests that they may be influenced by individual perceptions. Research suggests that perceptions influence behavior (Campbell 1999; Chen and Monroe 1998), which, in turn, suggests that a manager's perceptions about an asset's prospective utility may influence whether the manager decides to continue using the existing asset or invest in a replacement asset. I expect that depreciation method-induced differences in managers' prospective utility perceptions influence their asset replacement decisions, such that an asset with higher perceived prospective utility is less likely to be

replaced than an asset with lower perceived prospective utility. Hypothesis 4, stated in alternative form, is as follows:

H4: Depreciation method-induced differences in managers' prospective utility perceptions influence their asset replacement decisions, such that an asset with higher perceived prospective utility is less likely to be replaced than an asset with lower perceived prospective utility.

Other Relationships

I do not formulate hypotheses about (1) the relationship between managers' retrospective utility perceptions and managers' asset replacement decisions or (2) the relationship between firms' depreciation method choice and managers' prospective utility perceptions (see the dotted lines in Figure 1 which I discuss in Section IV). My rationale with respect to the first relation is that retrospective utility is an inherently backward-looking construct, so it should have little direct relation with managers forward-looking decisions (particularly when that relation is examined in the presence of managers' prospective utility perceptions). My rationale with respect to the second relation is that a firm's depreciation method choice, which is concerned with allocating past costs, should have little direct relation with managers' perceptions about an asset's future utility (particularly when that relation is examined in the presence of managers' retrospective utility perceptions). Although I do not formulate hypotheses about these two relationships, I nonetheless examine whether they are significant in Section IV.

III. TESTS OF HYPOTHESIS 1

In this section, I describe Experiment 1 and discuss the results of that experiment. I then describe Experiments 2 and 3 and discuss the results of those experiments. Experiment 1 also captures the data used to test H2 through H4. Tests of these hypotheses are discussed in Section IV.

Experiment 1

Participants

Participants consist of 205 M.B.A. students from two large public universities. The mean (median) age of participants is 28.97 (28.00) years, and the mean (median) years of professional work experience is 5.98 (4.00). Approximately 73 percent of the participants are male. The experiment was administered near the completion of participants' first year of coursework, so they had completed coursework in accounting, economics, finance, and management.⁶

Experimental Materials

The Appendix provides relevant portions of the research instrument used in Experiment 1 (the Appendix provides the straight-line depreciation condition). In Part I of the Appendix, the experimental materials place participants in the position of the "general manager of production" of a manufacturing company (Part I is discussed in more detail below). In Part II of the Appendix, participants are informed that the manager is responsible for investigating and resolving production variances. Participants are also informed that negative production variances have arisen in the current year and that the most likely cause of the

⁶ To evaluate whether participants are successfully randomized across experimental conditions, I examine whether the demographic variables (i.e., age, years of work experience, and gender) differ across experimental conditions. The differences are insignificant ($p\text{-value} > 0.10$).

variances is that employees are having difficulty operating machinery that is used to fabricate newly designed metal alloy components.

In Part III of the Appendix, participants are informed that the negative production variances cost the company \$34,000 each year. The manager is considering two options for dealing with the negative production variances. The first option is to replace the existing machinery with new machinery. The new machinery would be leased for six years, but must be returned to the leasing company at the end of the sixth year. This option has an 80 percent probability of eliminating the negative production variances, and would cost the company \$22,000 each year. The second option is to keep the existing machinery, but increase employee training and machinery maintenance. By doing so, the existing machinery could be used for six more years, but would be worthless at the end of that period. This option also has an 80 percent probability of eliminating the negative production variances, and would cost the company \$22,000 each year.

Importantly, both options for dealing with the negative production variances have identical costs, identical durations, and identical probabilities of eliminating the negative production variances. Further, undertaking either option is economically preferable to undertaking neither option. However, if new machinery is leased, then the existing machinery can be sold for \$43,000. *Therefore, the normatively correct choice is to lease the new machinery.*

The firm's depreciation method choice is manipulated between-subjects at two levels. In the accelerated depreciation method condition, historical cost is \$100,000 and accumulated depreciation is \$74,000, resulting in a book value of \$26,000. In the straight-line depreciation method condition, historical cost is \$100,000 and accumulated depreciation is \$40,000, resulting in a book value of \$60,000. Participants are informed that if the first option is selected, the existing machinery would be sold for \$43,000, which would result in a \$17,000 gain in the accelerated depreciation condition and a \$17,000 loss in the straight-line depreciation condition (see Part IV of the Appendix).

Returning to Part I of the Appendix, participants are also told the following: "Your compensation consists of fixed salary plus an annual bonus based on how well you control negative production variances. The Company's net income has no effect on your compensation." An alternative approach would be to inform participants that the company's net income does effect their compensation. However, this alternative approach would not be very interesting in an experimental context because earnings considerations would almost certainly dominate participants' decision processes, leaving little doubt that participants would make decisions that maximize earnings.⁷ In addition, because most of the participants in this study have previous experience as non-executive managers rather than executive managers, the absence of a link between firm earnings and compensation reflects the conditions under which most participants have previously worked.

The dependent variable is participants' preference to replace the existing machinery or continue using the existing machinery. As shown in Part V of the Appendix, responses are provided on a ten-point scale ranging from 1 ("I would definitely replace the existing machinery") to 10 ("I would definitely keep the existing machinery"). I also analyze participants' responses to this question by converting them to a binary variable. The binary variable takes on a value of 0 for capital investment preferences of 1 through 5 on the ten-point response scale and a value of 1 for capital investment preferences of 6 through 10

⁷ However, as discussed in Section II, participants may consider the earnings consequences of their decisions even in the absence of earnings-based compensation.

on the ten-point response scale.⁸ Also, for purposes of testing H2 through H4 (discussed in Section IV), participants respond to eight questions on a seven-point Likert scale that measure their perceptions about the machinery's retrospective utility and prospective utility. Finally, participants complete a post-experimental questionnaire, which contains manipulation checks and demographic questions.

A potential concern about obtaining participants' capital investment decisions at the time that I also obtain their perceptions about the existing machinery's retrospective and prospective utility is that the order in which the responses are obtained may influence how participants respond to the dependent variable. To avoid this problem, the order in which the dependent variable and the perception questions are presented is varied so that half of the participants respond to the dependent variable first and half of the participants respond to the perception questions first. To test for order effects, I estimate an ANOVA with participants' capital investment decisions as the dependent variable and the firm's depreciation method choice as the first independent variable and the order of presentation as the second independent variable. The main effect of order is insignificant ($p\text{-value} > 0.20$) as is the interaction between the firm's depreciation method choice and order ($p\text{-value} > 0.20$).

Manipulation Checks

The firm's depreciation method choice is manipulated between-subjects, although this variable also determines the existing machinery's book value and any gain or loss on the disposal of the existing machinery. Thus, the post-experimental questionnaire has participants respond to three manipulation checks, which take the form of multiple choice questions. These questions focus on (1) the firm's depreciation method choice, (2) the existing machinery's book value, and (3) the gain or loss on disposal of the existing machinery. Of the 205 participants, 196 participants (96 percent) answered the first question correctly, 197 participants (96 percent) answered the second question correctly, and 180 participants (88 percent) answered the third question correctly. The inferences and conclusions of this experiment are unaffected by excluding participants who incorrectly responded to any of these three questions.

Results of Experiment 1

Panel A of Table 1 provides tests of the total effect of firms' depreciation method choice on participants' capital investment decisions across the accelerated depreciation condition and straight-line depreciation condition.⁹ In the accelerated depreciation condition the mean (median) capital investment decision on the ten-point response scale is 4.60 (3.00) (indicating a tendency to replace existing machinery), while in the straight-line depreciation condition the mean (median) capital investment decision on the ten-point response scale is 6.47 (8.00) (indicating a tendency to keep existing machinery). The difference in means is significant ($t\text{-statistic} = 4.59$, $p\text{-value} < 0.001$) as is the difference in medians ($Z\text{-statistic} = 4.21$, $p\text{-value} < 0.001$). In the accelerated depreciation condition the mean (median) binary capital investment decision on the ten-point response scale is 0.35 (0.00) (indicating a tendency to replace existing machinery), while in the straight-line depreciation condition the mean (median) binary capital investment decision on the ten-point response scale is 0.65 (1.00) (indicating a tendency to keep existing machinery). The difference in means is significant ($t\text{-statistic} = 4.60$, $p\text{-value} < 0.001$) as is the difference in medians ($Z\text{-statistic}$

⁸ For expositional simplicity, I use the phrase "capital investment decision" throughout this paper regardless of which dependent variable specification is being used.

⁹ Panels B and C of Table 1 are discussed later in connection with Experiments 2 and 3.

TABLE 1
Tests of the Effect of Firms' Depreciation Method Choice on Managers' Capital Investment Decisions

Capital Investment Decision	Accelerated Depreciation Condition				Straight-Line Depreciation Condition				Tests for Diff. in Means		Tests for Diff. in Medians	
	Mean	Median	S.D.	n	Mean	Median	S.D.	n	t-stat.	p-value	Z-stat.	p-value
Panel A: Gain on Disposal of \$17,000 for Accelerated Depreciation versus Loss on Disposal of \$17,000 for Straight-Line Depreciation (Experiment 1)												
Capital inv. decision	4.60	3.00	2.86	98	6.47	8.00	2.95	107	4.59	<0.001	4.21	<0.001
Binary capital inv. decision	0.35	0.00	0.48	98	0.65	1.00	0.47	107	4.60	<0.001	4.38	<0.001
Panel B: Gain on Disposal of \$34,010 for Accelerated Depreciation versus Gain on Disposal of \$10 for Straight-Line Depreciation (Experiment 2)												
Capital inv. decision	3.28	2.00	2.62	50	5.23	5.00	2.83	51	3.60	<0.001	3.62	<0.001
Binary capital inv. decision	0.22	0.00	0.42	50	0.47	0.00	0.50	51	2.72	0.008	2.62	0.009
Panel C: Loss on Disposal of \$10 for Accelerated Depreciation versus Loss on Disposal of \$34,010 for Straight-Line Depreciation (Experiment 3)												
Capital inv. decision	4.29	3.50	2.46	52	6.02	8.00	3.12	51	3.12	0.002	3.02	0.003
Binary capital inv. decision	0.40	0.00	0.50	52	0.65	1.00	0.48	51	2.52	0.013	2.46	0.014

The columnar partition is the independent variable, which is manipulated between-subjects. The capital investment decision (i.e., the dependent variable) is participants' response on a ten-point scale ranging from 1 (I would definitely replace the existing machinery) to 10 (I would definitely keep the existing machinery). The binary capital investment decision takes on a value of 0 for capital investment decisions of 1 through 5 on the ten-point response scale and a value of 1 for capital investment decision of 6 through 10 on the ten-point response scale.

The columnar partition is the independent variable, which is manipulated between-subjects. The capital investment decision (i.e., the dependent variable) is participants' response on a ten-point scale ranging from 1 (I would definitely replace the existing machinery) to 10 (I would definitely keep the existing machinery). The binary capital investment decision takes on a value of 0 for capital investment decisions of 1 through 5 on the ten-point response scale and a value of 1 for capital investment decision of 6 through 10 on the ten-point response scale.

= 4.38, $p\text{-value} < 0.001$). Thus, the results reported in Panel A of Table 1 provide support for H1.

Experiments 2 and 3

Purpose of Experiments

The purpose of conducting Experiments 2 and 3 is to explore H1 in more detail. Recall that Experiment 1 focuses on the circumstance in which the firm's depreciation method choice resulted in either a gain on disposal (accelerated depreciation condition) or a loss on disposal (straight-line depreciation condition). It is also possible for the depreciation methods to yield (1) gains on disposal of different magnitudes (i.e., a large gain on disposal when accelerated depreciation is used versus a small gain on disposal when straight-line depreciation is used) and (2) losses on disposal of different magnitudes (i.e., a small loss on disposal when accelerated depreciation is used versus a large loss on disposal when straight-line depreciation is used). Thus, the purpose of Experiment 2 is to explore whether *gains* of different magnitudes influence managers' capital investment decisions, while the purpose of Experiment 3 is to explore whether *losses* of different magnitudes influence managers' capital investment decisions.

Participants

Participants in Experiments 2 and 3 consist of a total of 204 M.B.A. students from a large public university.¹⁰ These experiments were conducted on separate dates using different groups of M.B.A. students. The mean (median) age of participants is 29.30 (28.00) years, and the mean (median) years of professional work experience is 6.61 (5.00). Approximately 72 percent of the participants are male. The experiments were administered after participants' had completed coursework in accounting, economics, finance, and management.¹¹

Experimental Materials

The materials used in Experiments 2 and 3 are similar to the materials used in Experiment 1 (the first three paragraphs in the section titled "Experimental Materials" for Experiment 1 apply to Experiments 2 and 3 except for the selling price of the existing machinery so they are not repeated here). Experiments 2 and 3 differ from Experiment 1 primarily in terms of the selling price for which the existing machinery can be sold.¹² In Experiment 2, participants are informed that if they elect to acquire new machinery, the existing machinery would be sold for \$60,010, which would result in a "large gain of \$34,010" in the accelerated depreciation condition and a "very small gain of \$10" in the straight-line depreciation condition. Similarly, in Experiment 3, participants are informed that if they elect to acquire new machinery, the existing machinery would be sold for \$25,990, which would result in a "very small loss of \$10" in the accelerated depreciation condition and a "large loss of \$34,010" in the straight-line depreciation condition.¹³

¹⁰ For brevity, demographic information for Experiments 2 and 3 is discussed in combined form. However, demographic information for the two experiments is similar.

¹¹ To evaluate whether participants are successfully randomized across experimental conditions, I examine whether the demographic variables (i.e., age, years of work experience, and gender) differ across experimental conditions. The differences are insignificant ($p\text{-value} > 0.10$).

¹² As discussed at the beginning of Section IV, the materials for Experiment 1 also differ from the materials for Experiments 2 and 3 in that Experiment 1 also collects certain additional information used to test H2 through H4.

¹³ The purpose of using the language a "very small gain of \$10" or a "very small loss of \$10" rather than a "gain of \$10" or a "loss of \$10" is to emphasize the point that the gain or loss is of a trivial magnitude.

As in Experiment 1, the dependent variable in Experiments 2 and 3 is participants' preference to replace the existing machinery or continue using the existing machinery. Responses are provided on the same ten-point scale used in Experiment 1. I also analyze participants' responses to this question by converting them to a binary variable. After responding to the dependent variable, participants complete a post-experimental questionnaire, which contains manipulation checks and demographic questions.

Manipulation Checks

The post-experimental questionnaire in Experiments 2 and 3 has participants respond to three manipulation checks, which take the form of multiple choice questions. These questions focus on (1) the firm's depreciation method choice, (2) the existing machinery's book value, and (3) the gain or loss on disposal of the existing machinery. Of the 101 participants in Experiment 2, 90 participants (89 percent) answered the first question correctly, 97 participants (96 percent) answered the second question correctly, and 93 participants (92 percent) answered the third question correctly. Of the 103 participants in Experiment 3, 91 participants (88 percent) answered the first question correctly, 101 participants (98 percent) answered the second question correctly, and 100 participants (97 percent) answered the third question correctly. The inferences and conclusions of these experiments are unaffected by excluding participants who incorrectly responded to any of these three questions.

Results of Experiments 2 and 3

In the context of two differentially sized gains (Experiment 2), Panel B of Table 1 provides tests of the total effect of firms' depreciation method choice on participants' capital investment decisions across the accelerated depreciation condition and straight-line depreciation condition. In the accelerated depreciation condition the mean (median) capital investment decision on the ten-point response scale is 3.28 (2.00) (indicating a tendency to replace existing machinery), while in the straight-line depreciation condition the mean (median) capital investment decision on the ten-point response scale is 5.23 (5.00) (indicating some ambivalence about whether to replace or keep existing machinery). The difference in means is significant (t -statistic = 3.60, p -value < 0.001) as is the difference in medians (Z -statistic = 3.62, p -value < 0.001). In the accelerated depreciation condition the mean (median) binary capital investment decision on the ten-point response scale is 0.22 (0.00) (indicating a tendency to replace existing machinery), while in the straight-line depreciation condition the mean (median) binary capital investment decision on the ten-point response scale is 0.47 (0.00) (indicating some ambivalence about whether to replace or keep existing machinery). The difference in means is significant (t -statistic = 2.72, p -value = 0.008) as is the difference in medians (Z -statistic = 2.62, p -value = 0.009). Thus, the results reported in Panel B of Table 1 provide support for H1 in the context of two differentially sized gains.

In the context of two differentially sized losses (Experiment 3), Panel C of Table 1 provides tests of the total effect of firms' depreciation method choice on participants' capital investment decisions across the accelerated depreciation condition and straight-line depreciation condition. In the accelerated depreciation condition the mean (median) capital investment decision on the ten-point response scale is 4.29 (3.50) (indicating a tendency to replace existing machinery), while in the straight-line depreciation condition the mean (median) capital investment decision on the ten-point response scale is 6.02 (8.00) (indicating a tendency to keep existing machinery). The difference in means is significant (t -statistic = 3.12, p -value = 0.002) as is the difference in medians (Z -statistic = 3.02, p -value

= 0.003). In the accelerated depreciation condition the mean (median) binary capital investment decision on the ten-point response scale is 0.40 (0.00) (indicating a tendency to replace existing machinery), while in the straight-line depreciation condition the mean (median) binary capital investment decision on the ten-point response scale is 0.65 (1.00) (indicating a tendency to keep existing machinery). The difference in means is significant (t -statistic = 2.52, p -value = 0.013) as is the difference in medians (Z -statistic = 2.46, p -value = 0.014). Thus, the results reported in Panel C of Table 1 provide support for H1 in the context of two differentially sized losses.

Finally, notice that the mean (median) capital investment decision on the ten-point response scale of 5.23 (5.00) for the "very small gain of \$10" in Panel B is greater than the mean (median) capital investment decision on the ten-point response scale of 4.29 (3.50) for the "very small loss of \$10" in Panel C (p -value = 0.073 at the mean and p -value = 0.061 at the median). This difference is unusual because it indicates that participants are more likely to replace the existing machinery when there is a "very small loss of \$10" on disposal (coupled with a selling price of \$25,990 for the machinery) than when there is a "very small gain of \$10" on disposal (coupled with a selling price of \$60,010 for the machinery).¹⁴ However, the mean (median) binary capital investment decision of 0.47 (0.00) for the "very small gain of \$10" in Panel B is statistically indistinguishable from the binary capital investment decision of 0.40 (0.00) for the "very small loss of \$10" in Panel C (p -value = 0.501 at the mean and p -value = 0.499 at the median). Further, the mean (median) capital investment decision on the ten-point response scale of 3.28 (2.00) for the "large gain of \$34,010" in Panel B is less than the mean (median) capital investment decision on the ten-point response scale of 6.02 (8.00) for the "large loss of \$34,010" in Panel C (p -value < 0.001 at the mean and p -value < 0.001 at the median). This difference is as one would expect (i.e., participants are more likely to replace the existing machinery when there is a large gain on disposal than when there is a large loss on disposal). A similar finding arises for the mean (median) binary capital investment decision.

IV. TESTS OF HYPOTHESES 2 THROUGH 4

The experimental materials for Experiment 1 also require participants to respond to questions that measure their perceptions about the existing machinery's retrospective utility and prospective utility. Participants' responses to these questions are used to test H2 through H4.¹⁵

Retrospective and Prospective Utility

Retrospective utility and prospective utility are latent (unobservable) constructs that must be measured. However, I am not aware of any previously developed measurement scales for these constructs. As a result, I formulate an initial set of descriptors for retrospective utility and an initial set of descriptors for prospective utility (six descriptors for

¹⁴ A potential reason for these marginally significant differences is that Experiments 2 and 3 were conducted on separate dates using different groups of M.B.A. students. As a result, there was not random assignment of subjects between Experiments 2 and 3 (there was random assignment of subjects between the conditions comprising each of these individual experiments). In the absence of random assignment between Experiments 2 and 3, inferences about the aforementioned differences should be made with caution.

¹⁵ In each of the three experiments discussed in Section III, the firm's depreciation method choice is manipulated in an identical fashion, and it is this manipulation that is linked to managers' perceptions of the machinery's retrospective and prospective utility. Because the variable that affects managers' utility perceptions is identical in all three experiments, it is unnecessary to collect managers' utility perceptions in each experiment. As a result, managers' perceptions of retrospective and prospective utility are collected and analyzed in Experiment 1 only.

each construct). The initial 12 descriptors were evaluated by seven individuals (four doctoral students and three accounting faculty) for overall clarity. Of the 12 initial descriptors, four descriptors were eliminated (they were deemed to be somewhat unclear by three or more individuals) and two descriptors were significantly rephrased. This resulted in four descriptors for retrospective utility and four descriptors for prospective utility.

The reasonableness of the construct descriptors was evaluated by having 21 graduate business students sort the eight descriptors based upon whether they primarily focus on the (1) past benefits that have already been provided or (2) future benefits that will be provided. The graduate students classified the descriptors in a manner consistent with the construct label approximately 88 percent of the time. The four descriptors in Panel A of Exhibit 1 are intended to reflect perceptions of retrospective utility, while the four descriptors in Panel B of Exhibit 1 are intended to reflect perceptions of prospective utility. All of the items have been reverse-scored so that higher numbered responses indicate greater perceived utility.

Table 2 provides descriptive statistics for the items that are intended to reflect participants' perceptions of retrospective utility and prospective utility. Participants' mean (median) response to the retrospective utility items in the accelerated depreciation condition is 3.91 (3.88), while participants' mean (median) response to those items in the straight-line depreciation condition is 3.29 (3.25). The difference in means is significant (t -statistic = -3.77 , p -value < 0.001) as is the difference in medians (Z -statistic = -3.69 , p -value < 0.001). Participants' mean (median) response to the prospective utility items in the accelerated depreciation condition is 4.10 (4.00), while participants' mean (median) response to those items in the straight-line depreciation condition is 4.45 (4.75). The difference in means is marginally significant (t -statistic = 1.82, p -value = 0.070) as is the difference in medians (Z -statistic = 1.88, p -value = 0.060).

Confirmatory Factor Analysis

I use confirmatory factor analysis to assess whether the items shown in Exhibit 1 adequately reflect managers' perceptions of retrospective utility and prospective utility.¹⁶ Initial analyses reveal that one of the retrospective utility items (Question 1 in Exhibit 1) and one of the prospective utility items (Question 6 in Exhibit 1) cross-load on the other construct. As a result, these two items were dropped from the analyses, resulting in three items that reflect perceptions of retrospective utility and three items that reflect perceptions of prospective utility. Keeping the items that cross-load does not alter any conclusions.

To assess whether the remaining utility items reflect two separate utility constructs, I focus on three commonly used fit statistics, each of which should ideally exceed 0.90 (Hatcher 1994). When the items are treated as two utility constructs, the goodness of fit index is 0.93, Bentler's comparative fit index is 0.95, and Bentler and Bonnett's non-normed fit index is 0.90. However, when the utility items are treated as a single combined construct, the goodness of fit index is 0.85, Bentler's comparative fit index is 0.87, and Bentler and Bonnett's non-normed fit index is 0.79. Based on these analyses, I conclude that the utility items reflect two separate constructs rather than one combined construct, as expected.

Next, I examine the (1) standardized factor loadings, (2) item reliabilities, (3) composite reliabilities, and (4) variance extracted estimates. As shown in Table 3, the standardized

¹⁶ When the items intended to reflect a construct are posited *a priori* (which is the case in this study), confirmatory factor analysis is used (Hatcher 1994; Sharma 1996). In contrast, when the items intended to reflect a construct are *not* posited *a priori*, exploratory factor analysis is used (Hatcher 1994; Sharma 1996).

EXHIBIT 1
Items that Reflect Perceptions of Retrospective Utility and Prospective Utility

Panel A: Items that Reflect Perceptions of Retrospective Utility

- 1. I feel that the existing machinery has provided substantial past benefits.
- 2. I feel that most of the available benefits from the existing machinery were provided in the past.
- 3. I feel that the existing machinery already provided more benefits in the past than it is capable of providing in the future.
- 4. I feel that the existing machinery is pretty much used up.

Panel B: Items that Reflect Perceptions of Prospective Utility

- 5. I feel that most of the available benefits from the existing machinery will be provided in the future.
- 6. I feel that the existing machinery is capable of providing more benefits in the future than it has already provided in the past.
- 7. I feel that the existing machinery can provide substantial future benefits.
- 8. I feel that the existing machinery can be used effectively for many more years in the future.

Participants' responses are given on a seven-point Likert scale, with the left end of the scale labeled as "Definitely Yes" and the right end of the scale labeled as "Definitely Not." All of the items have been reverse scored so that higher numbered responses indicate greater perceived utility.

TABLE 2
Descriptive Statistics for Retrospective and Prospective Utility Items

Capital Investment Decision	Accelerated Depreciation Condition (n = 98)		Straight-line Depreciation Condition (n = 107)		Test for Diff. in Means		Test for Diff. in Medians	
	Mean	Median	Mean	Median	t-stat.	p-value	Z-stat.	p-value
Retrospective utility								
Item 1	4.56	5.00	4.05	4.00	-2.46	0.015	-2.41	0.016
Item 2	4.09	4.00	3.33	3.00	-3.46	<0.001	-3.36	<0.001
Item 3	3.81	3.00	3.29	3.00	-2.32	0.021	-2.47	0.014
Item 4	3.17	3.00	2.47	2.00	-3.16	0.002	-3.19	0.001
Items 1-4	3.91	3.88	3.29	3.25	-3.77	<0.001	-3.69	<0.001
Prospective utility								
Item 5	3.60	3.00	4.09	4.00	2.28	0.023	2.27	0.023
Item 6	4.02	4.00	4.39	5.00	1.48	0.140	1.51	0.132
Item 7	4.42	5.00	4.60	5.00	0.84	0.404	0.88	0.378
Item 8	4.36	4.00	4.70	5.00	1.56	0.121	1.78	0.075
Items 5-8	4.10	4.00	4.45	4.75	1.82	0.070	1.88	0.060

Participants' responses are given on a seven-point Likert scale, with the left end of the scale labeled as "Definitely Yes" and the right end of the scale labeled as "Definitely Not." All of the items have been reverse scored so that higher numbered responses indicate greater perceived utility.

See Exhibit 1 for the utility items.

factor loadings range from 0.65 to 0.94, all of which are acceptable (Hatcher 1994). The related t-statistics indicate that the standardized factor loadings are all significantly different from zero (p-value < 0.001). The reliabilities for the individual items range from 0.42 to

TABLE 3
Standardized Factor Loadings (n = 205)

Variable	Standardized Factor Loading	t-statistic	p-value	Reliability	Composite Reliability	Variance Extracted
Retrospective utility					0.87	0.69
Item 2	0.81	13.54	<0.001	0.66		
Item 3	0.93	16.87	<0.001	0.86		
Item 4	0.75	12.07	<0.001	0.56		
Prospective utility					0.85	0.66
Item 5	0.65	10.12	<0.001	0.42		
Item 7	0.94	17.13	<0.001	0.88		
Item 8	0.82	13.93	<0.001	0.67		

See Exhibit 1 for the utility items and discussion under the heading "Confirmatory Factor Analysis" in Section IV.

0.88, and the composite reliabilities for the retrospective utility items and the prospective utility items are 0.87 and 0.85, respectively.¹⁷ These composite reliabilities exceed the minimum reliability threshold of 0.70 (Hatcher 1994). The variance extracted estimates for the retrospective utility items and the prospective utility items are 0.69 and 0.66, respectively. These estimates, which exceed the minimum threshold of 0.50 (Hatcher 1994), assess the amount of variance captured by the underlying factors in relation to the amount of variance due to measurement error (Fornell and Larcker 1981).

Tests of Hypotheses

To test H2 through H4 (and to test the significance of the indirect/mediated path that these hypotheses collectively predict), I follow the procedures in Taylor et al. (2006), which involve estimating the following regressions:^{18,19}

$$RETRO = \delta_1 + \beta_1 DEPR + \epsilon; \quad (1)$$

$$INVEST = \delta_2 + \beta_2 DEPR + \epsilon; \quad (2)$$

$$PROSP = \delta_3 + \beta_3 DEPR + \beta_4 RETRO + \epsilon; \quad (3)$$

$$INVEST = \delta_4 + \beta_5 RETRO + \beta_6 PROSP + \epsilon; \quad (4)$$

$$INVEST = \delta_5 + \beta_7 DEPR + \beta_8 RETRO + \beta_9 PROSP + \epsilon; \quad (5)$$

where *RETRO* is participants' factor scores for the retrospective utility items shown in Table 3; *DEPR* is the firm's depreciation method choice, which is coded as 0 for participants in

¹⁷ To compute the composite reliabilities, I use the approach in Fornell and Larcker (1981).

¹⁸ The mediation procedures in Baron and Kenny (1986) are for a two-path mediated effect. Hypotheses 2 through 4 collectively predict a three-path mediated effect so the procedures in Baron and Kenny (1986) are not appropriate here. However, Taylor et al. (2006) extend the work of Baron and Kenny (1986) to a three-path mediation context.

¹⁹ Equation (4) is reported for informational purposes only (it need not be estimated to test for mediation).

the accelerated depreciation condition and 1 for participants in the straight-line depreciation condition; *INVEST* is participants' capital investment decision, which is provided on a ten-point scale ranging from 1 ("I would definitely replace the existing machinery") to 10 ("I would definitely keep the existing machinery");²⁰ *PROSP* is participants' factor scores for the prospective utility items shown in Table 3.²¹

The data is standardized so that variables have a mean value of zero and a variance of unity. Standardizing the data (1) puts the variables on an equal basis (i.e., their effect is not measured in terms of the original units but in terms of standard deviation units that are free of location and scale attributes) and (2) enables direct comparison of the regression coefficients (i.e., larger absolute regression coefficients imply greater contribution to the explanation of the dependent variable) (Gujarati 2003). When regressions are estimated using standardized data, the intercepts drop out and the coefficients are partial correlations.²²

Table 4 reports the regression results for Equations (1) through (5), and Figure 1 summarizes those results (the dotted lines in Figure 1 represent paths that were not hypothesized but are reported for informational purposes). Hypothesis 2 predicts that managers perceive that machinery that has been depreciated using straight-line depreciation has provided less retrospective utility than machinery that has been depreciated using accelerated depreciation. As shown in Table 4, the results of estimating Equation (1) reveal that the path coefficient between the firm's depreciation method choice and managers' perceptions about the machinery's retrospective utility is negative and significant (t-statistic = -3.45, p-value < 0.001), which supports H2.

Hypothesis 3 predicts that managers' perceptions about the machinery's retrospective utility are inversely related to their perceptions about the machinery's prospective utility. As shown in Table 4, the results of estimating Equation (3) reveal that the path coefficient between managers' perceptions of the machinery's retrospective utility and their perceptions of the machinery's prospective utility is negative and significant (t-statistic = -15.70, p-value < 0.001), which supports H3.

Hypothesis 4 predicts that depreciation method-induced differences in managers' prospective utility perceptions influence their capital investment decisions, such that machinery with higher perceived prospective utility is less likely to be replaced than machinery with lower perceived prospective utility. As shown in Table 4, the results of estimating Equation (5) reveal that the path coefficient between managers' perceptions of the machinery's prospective utility and their capital investment decision is positive and significant (t-statistic = 4.81, p-value < 0.001), which supports H4.

The regression results in Table 4 also allow for the estimation of the total effect, direct effect, and mediated effect. The total effect is 0.31 (as shown in Equation (2) in Table 4),²³ the direct effect is 0.23 (as shown in Equation (5) in Table 4), and the mediated effect is 0.08 (this is computed as the difference between the total effect and the direct effect). The portion of the total effect that is mediated is approximately 26 percent (0.08/0.31), and

²⁰ *INVEST* may also be coded in its binary form for purposes of testing the hypotheses. Regardless of which specification is used, the conclusions of the analyses are the same. For brevity, I report and discuss the analyses using the continuous form of *INVEST* only.

²¹ The results of using an alternative methodology (i.e., structural equation modeling with latent variables) are discussed at the end of this section.

²² Inferences are identical when non-standardized data are used.

²³ Using non-standardized data, the total effect of 1.87 can also be computed using mean values in Panel A of Table 1 (the mean of 6.47 in the straight-line depreciation condition minus the mean of 4.60 in the accelerated depreciation condition).

TABLE 4
Regressions to Test H2 through H4 (n = 205)

Equation	Dependent Variable	Coefficient (t-statistic) [p-value]			Model F	R ² (%)
		DEPR	RETRO	PROSP		
Equation (1)	RETRO	-0.24 (-3.45) [<0.001]			11.90	5.54
Equation (2)	INVEST	0.31 (4.59) [<0.001]			21.05	9.40
Equation (3)	PROSP	-0.06 (-1.17) [0.245]	-0.76 (-15.70) [<0.001]		126.64	55.63
Equation (4)	INVEST		-0.19 (-2.09) [0.038]	0.39 (4.35) [<0.001]	41.27	29.01
Equation (5)	INVEST	0.23 (3.89) [<0.001]	-0.11 (-1.26) [0.209]	0.41 (4.81) [<0.001]	34.47	33.97

The following regressions are estimated:

$RETRO = \beta_1 DEPR + \epsilon;$ (1)

$INVEST = \beta_2 DEPR + \epsilon;$ (2)

$PROSP = \beta_3 DEPR + \beta_4 RETRO + \epsilon;$ (3)

$INVEST = \beta_5 RETRO + \beta_6 PROSP + \epsilon;$ (4)

$INVEST = \beta_7 DEPR + \beta_8 RETRO + \beta_9 PROSP + \epsilon;$ (5)

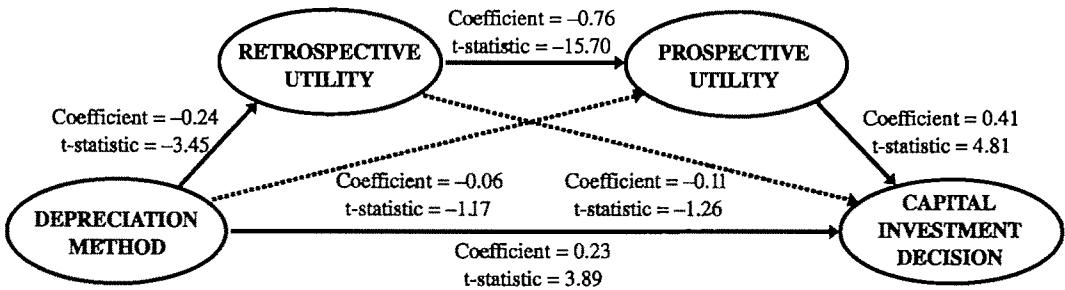
where *RETRO* is participants' factor scores for the retrospective utility items shown in Table 3; *DEPR* is the firm's depreciation method choice, which is coded as 0 for participants in the accelerated depreciation condition and 1 for participants in the straight-line depreciation condition; *INVEST* is participants' capital investment decision, which is provided on a ten-point scale ranging from 1 (I would definitely replace the existing machinery) to 10 (I would definitely keep the existing machinery); *PROSP* is participants' factor scores for the prospective utility items shown in Table 3. Equation (4) is reported for informational purposes only (it need not be estimated to test for mediation or to measure the mediated effect). The data is standardized so that variables have a mean value of zero and a variance of unity.

the mediated effect is statistically significant (p-value < 0.01). Thus, not only are H2 through H4 supported, but there is a significant three-path mediated effect.

Four additional matters deserve elaboration. First, the regression results confirm that the interior paths are insignificant (see discussion following H4). Notice that the coefficient on *DEPR* is insignificant in Equation (3) (t-statistic = -1.17, p-value = 0.245) as is the coefficient on *RETRO* in Equation (5) (t-statistic = -1.26, p-value = 0.209). The lack of significance for the two interior paths suggests that neither *RETRO* nor *PROSP* are suitable mediators *on an individual basis*. Instead, it appears that the model should include both utility constructs.

Second, Equation (5) reveals that *DEPR* remains significant in the presence of *RETRO* and *PROSP*, which indicates that there is partial mediation rather than full mediation. As noted by Baron and Kenny (1986), when a dependent variable has multiple causes (which is likely to be the case in the present context), it may not be realistic to find evidence of

FIGURE 1
Hypothesized Model with Path Estimates



The independent variable is the firm's "Depreciation Method," which is manipulated between-subjects at two levels and is coded as 0 for participants in the accelerated depreciation condition and 1 for participants in the straight-line depreciation condition. "Retrospective Utility" is participants' factor scores for the retrospective utility items shown in Table 3. "Prospective Utility" is participants' factor scores for the prospective utility items shown in Table 3. The dependent variable is "Capital Investment Decision," which is participants' response on a ten-point scale ranging from 1 (I would definitely replace the existing machinery) to 10 (I would definitely keep the existing machinery). The solid lines represent relationships that were hypothesized, while the dotted lines represent relationships that were not hypothesized. Standardized beta weights are indicated for each path.

full mediation using one or a few variables. In addition, it should be noted that retrospective and prospective utility perceptions are internal psychological variables that are neither directly observable nor precisely measurable, which increases measurement error and reduces the likelihood of finding full mediation.

Third, the items that comprise retrospective and prospective utility could be combined into a single construct. I find that when the two utility constructs are combined into a single unidimensional construct, the indirect path remains significant. Further, I continue to find evidence of partial mediation rather than full mediation, and the magnitude of the mediated effect is similar to that reported above.

Fourth, the hypotheses in this study could also be tested using a more complicated statistical procedure—structural equation modeling with latent variables (see Hatcher 1994; Sharma 1996). Although I opt to use the approach in Taylor et al. (2006), as discussed above, the conclusions of this study are invariant to the methodology chosen. However, when I use structural equation modeling with latent variables, the t-statistics for the hypothesized paths in Figure 1 are smaller but remain highly significant ($p < 0.01$).

V. ALTERNATIVE EXPLANATIONS

In this section, I discuss and evaluate alternative explanations for the experimental findings. The first alternative explanation is that the participants misunderstand the meaning of accounting depreciation. That is, despite the information specifically stated in the experimental materials and the information specifically written in their textbooks and then reinforced during formal classroom instruction, participants simply misunderstand the nature of accounting depreciation (i.e., they believe that accounting depreciation is a method of asset valuation or a method of physical condition determination). To deal with this concern, Experiment 1 was administered to 88 junior- and senior-level accounting students

who were enrolled in Intermediate Accounting II.²⁴ If the results of Experiment 1 using M.B.A. students are driven by a claimed misunderstanding about the meaning of accounting depreciation, then the results should disappear when subjects are well versed in the meaning of accounting depreciation. Using this alternative group of subjects, I find support for all of the hypotheses in this study, despite the fact that the participants have in-depth knowledge about the meaning of accounting depreciation.

The second alternative explanation is that participants were unable to identify the normatively correct decision, which is to invest in the replacement machinery. To evaluate this possibility, I modify the research instrument used in Experiment 1 by removing all information related to accounting depreciation. Twenty M.B.A. students completed the abbreviated instrument. The mean response to the dependent variable is 3.10, which suggests that participants understand that investing in the replacement machinery is the economically efficient decision.

The third alternative explanation is that the firm's depreciation method choice conveys information about the existing machinery's *physical condition*, which, in turn, influences managers' capital investment decisions. To ensure that the firm's depreciation method choice does not cause participants to misconstrue the existing machinery's physical condition, I include identical information in the experimental materials about the machinery's physical condition, original useful life, remaining useful life, and current age. Further, the post-experimental questionnaire contains multiple choice questions about these four factors to determine whether they are salient to participants. I find that participants answer each individual question correctly between 90 and 98 percent of the time, and the accuracy of participants' responses to the questions does not differ between experimental conditions ($p\text{-value} > 0.10$). Therefore, experimental differences in participants' capital investment decisions are not likely to be caused by incorrect impressions about what the firm's depreciation method choice implies about the physical condition of the machinery.

The fourth alternative explanation is that the firm's depreciation method choice conveys information about the deterioration profile of the asset. The experimental materials inform participants of the following: "The Company depreciates machines over their expected useful lives using the same depreciation method for all machines." This statement communicates to participants that the firm's depreciation method choice is a firm-wide policy that is not specifically linked to the deterioration profile of any specific asset. To evaluate whether participants interpret this statement in the manner intended, a multiple choice question in the post-experimental questionnaire related to the firm's depreciation method choice allows participants to choose an option that reads as follows: "Machines are depreciated over their expected useful lives using a depreciation method that corresponds to the physical deterioration of each individual machine." Only 6 percent of the participants selected this option, and the results of this study are unaffected by excluding those participants from the statistical analyses. Therefore, the experimental differences in participants' capital investment decisions are not likely to be caused by an incorrect belief that the firm's depreciation method choice is informative about the deterioration profile of the machinery.

The final alternative explanation is that the firm's depreciation method choice conveys information about the existing machinery's *market value*, which, in turn, influences managers' capital investment decisions. To ensure that participants do not misconstrue the machinery's market value, the experimental materials directly provide that information to

²⁴ The text used in students' intermediate accounting courses makes it amply clear that accounting depreciation is a systematic and rational method of cost allocation. The students in this class were first introduced to this notion in an introductory accounting course and then had this notion reinforced in Intermediate Accounting I.

participants. Further, the post-experimental questionnaire contains a multiple-choice question about the machinery's market value. I find that participants answered this question correctly 97 percent of the time, and the accuracy of their responses does not differ between experimental conditions ($p\text{-value} > 0.10$). Therefore, the experimental differences in participants' capital investment decisions are not likely to be caused by an incorrect belief about what the firm's depreciation method choice implies about the market value of the machinery.

VI. SUMMARY AND LIMITATIONS

In this study, I explore whether a seemingly innocuous choice made for external financial reporting purposes (i.e., the choice between straight-line depreciation and accelerated depreciation) causes non-executive managers to make non-value-maximizing capital investment decisions. The experimental setting involves a situation in which managers must decide whether to continue using an existing asset or invest in a replacement asset. Replacing the existing asset yields higher cash flows and managers are aware of this fact. However, if the asset is replaced, then the greater remaining book value under straight-line depreciation relative to accelerated depreciation causes earnings to be lower. Lower earnings and psychological forces may push managers of firms that use straight-line depreciation away from making the economically efficient capital investment decision.

The experimental results confirm my expectations about the effect of firms' depreciation method choice on non-executive managers' capital investment decisions. Specifically, I find that managers of firms that use straight-line depreciation are less likely to invest in replacement assets than are managers of firms that use accelerated depreciation. Further, I find that managers perceive that an asset depreciated using straight-line depreciation has provided less retrospective utility than an asset depreciated using accelerated depreciation. In turn, I find that depreciation-method-induced differences in managers' retrospective utility perceptions influence their prospective utility perceptions, which, in turn, influence managers' asset replacement decisions.

Efforts to understand the connection between firms' depreciation method choice and managers' capital investment decisions are of considerable practical importance because capital investment decisions impact the economic well being of firms, shareholders, and society. One implication of this study is that the use of straight-line depreciation may result in managers forgoing economically efficient capital investment opportunities, which, in turn, suggests that the use of accelerated depreciation might create a closer alignment between normative models of decision-making and the actual capital investment decisions of managers. Perhaps a logical way to address the problem associated with the use of straight-line depreciation is to deny managers access to depreciation-related information. As discussed in Section V, when depreciation-related information is omitted from the experimental materials, participants tend to make the economically efficient capital investment decision.

This study is subject to several limitations. First, the instruments used in the experiments necessarily contain less information than managers are likely to encounter in practice, and participants are not permitted to seek out or obtain other information that they deem potentially relevant. As a result, the behavior that I observe may not arise in natural settings. Second, the participants in this study are M.B.A. students, not practicing business managers, so it is possible that the results of this study may not arise if working business professionals who routinely make capital investment decisions are used as participants. This concern is mitigated by the fact that the participants are drawn from M.B.A. programs in which the work experience requirements are quite substantial. Third, while the experimental setting in this study involves a situation in which the economically efficient capital investment

decision is to invest in a replacement asset, there are situations in which the economically efficient capital investment decision is not to invest in a replacement asset. The results of this study do not address the latter circumstance.

APPENDIX

PORTIONS OF RESEARCH INSTRUMENT FOR EXPERIMENT 1 (STRAIGHT-LINE DEPRECIATION CONDITION)

I. BACKGROUND

Gatton Corporation (the “Company”) manufactures high-quality camping gear. You are the Company’s general manager of production. Your compensation consists of fixed salary plus an annual bonus based on how well you control negative production variances (this term is defined below). The Company’s net income has no effect on your compensation. Also, the Company’s financial condition is solid.

II. NEGATIVE PRODUCTION VARIANCES

You are investigating negative production variances, which occur when actual production costs exceed expected production costs. One of the production supervisors indicated that the machinery used to fabricate newly designed metal alloy components is difficult for employees to operate, resulting in excessive labor hours and materials waste. The supervisor’s explanation for the negative production variances is likely to be correct, but is not necessarily correct.

Four years ago, the Company purchased the production machinery that is currently in use. At the time of purchase, the machinery’s expected useful life was about seven years, although the actual useful life may be shorter or longer depending on production volume and the machinery’s durability.

III. OPTIONS FOR DEALING WITH NEGATIVE PRODUCTION VARIANCES

The negative production variances are \$34,000 this year, and will recur in the future unless corrective action is taken. Eliminating the production variances will save \$34,000 annually. You must choose between two options for dealing with the negative production variances.

OPTION 1: Replace the existing machinery with more advanced machinery acquired under a six-year lease. The machinery must be returned to the leasing company after six years, which is the machinery’s useful life. The annual lease payment would be \$22,000, which includes all maintenance costs. Leasing arrangements of this type will not be available in the future. The following are likelihood estimates for this option:

- Likelihood that this option WILL eliminate the negative production variances 80%
- Likelihood that this option WILL NOT eliminate the negative production variances 20%

OPTION 2: Keep the existing machinery and increase employee training and machinery maintenance. This option would also cost \$22,000 annually. According to reliable engineering reports, the existing machinery could be used for six more years if training and maintenance are increased. The machinery would have no value at the end of this period. The following are likelihood estimates for this option:

- Likelihood that this option WILL eliminate the negative production variances 80%
- Likelihood that this option WILL NOT eliminate the negative production variances 20%

IV. EXISTING MACHINERY

The Company depreciates machines over their expected useful lives using the same depreciation method for all machines (the straight-line method). The existing machinery has been used during the past four years and is in "average" condition. The machinery could be sold right now for \$43,000.

The accounting records indicate that the **BOOK VALUE of the existing machinery** is \$60,000, which is determined as follows:

ORIGINAL COST OF MACHINERY	\$100,000
LESS: ACCUMULATED DEPRECIATION	<u>\$ 40,000</u>
EQUALS: BOOK VALUE	<u>\$ 60,000</u>

Accumulated depreciation to date of \$40,000 is the amount that has been recorded as depreciation expense in the past. The book value of \$60,000 is the portion of the machinery's cost that will be recorded as depreciation expense in future years if the machinery is not replaced.

If the existing machinery is replaced, the book value of \$60,000 will be netted against the proceeds from sale of \$43,000 resulting in a LOSS of \$17,000 on the income statement. Disposing of the machinery would not affect the Company's taxes.

V. YOUR DECISION

Carefully consider the information discussed on Pages 1 and 2 (please feel free to review that information). Please circle **ONE** number on the scale below to indicate which of the two options discussed on Page 1 you would select.

Circling "1" indicates that you would definitely select **OPTION 1** (i.e., replace existing machinery), while circling "10" indicates that you would definitely select **OPTION 2** (i.e., keep existing machinery). Circling a number between "1" and "10" indicates the relative strength of your choice. Please make sure that you circle **ONE** number only.

	OPTION 1 (i.e., replace existing machinery)					OPTION 2 (i.e., keep existing machinery)					
I would definitely select	1	2	3	4	5	6	7	8	9	10	I would definitely select
OPTION 1											OPTION 2

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Beating Earnings Benchmarks and the Cost of Debt

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ABSTRACT: Prior research documents that firms tend to beat three earnings benchmarks—zero earnings, last year's earnings, and analyst's forecasted earnings—and that there are both equity market and compensation-related benefits associated with beating these benchmarks. This study investigates whether and under what conditions beating these three earnings benchmarks reduces a firm's cost of debt. I use two proxies for a firm's cost of debt: credit ratings and initial bond yield spread. Results suggest that firms beating earnings benchmarks have a higher probability of rating upgrades and a smaller initial bond yield spread. Additional analyses indicate that (1) the benefits of beating earnings benchmarks are more pronounced for firms with high default risk; (2) beating the zero earnings benchmark generally provides the biggest reward in terms of a lower cost of debt; and (3) the reduction in the cost of debt is attenuated but does not disappear for firms beating benchmarks through earnings management. In sum, results suggest that there are benefits associated with beating earnings benchmarks in the debt market. These benefits vary by benchmark, firm default risk, and method utilized to beat the benchmark. Among other implications, this evidence suggests that the relative importance of specific benchmarks differs across the equity and bond markets.

Keywords: *earnings benchmarks; cost of debt; credit ratings; yield spread.*

Data Availability: *Data are available from public sources identified in the paper.*

I. INTRODUCTION

Recent research documents that a disproportionate number of firms *barely* report profits, earnings increases over last year's earnings, and positive earnings surprises relative to analysts' expectations (Burgstahler and Dichev 1997; Degeorge et al. 1999; Hayn 1995; Burgstahler and Eames 2002). Burgstahler and Dichev (1997) and Degeorge et al. (1999) postulate that firms try to beat earnings benchmarks because firms'

This paper is based on my dissertation at the University of Georgia. I gratefully acknowledge the encouragement and guidance from the members of my dissertation committee, Steve Baginski, Linda Bamber, Chris Cornwell, Ken Gaver, and especially Ben Ayers (Chair). I appreciate the constructive comments of two anonymous reviewers, Michael Bamber, Scott Bronson, Richard Cantor, Dan Dhaliwal (editor), Ilia Dichev, Sandra Champlain, Jenny Gaver, Jackie Hammersley, Kathy Petroni, K. Ramesh, Partha Sengupta, Isabel Wang, Eric Yeung, and seminar participants at Arizona State University, the Chinese University of Hong Kong, Hong Kong University, HKUST, Michigan State University, Southern Methodist University, Texas Christian University, Tulane University, University of Georgia, University of Kansas, the University of Melbourne, University of Oklahoma, and the participants at the 2006 FARS Midyear Meeting.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted November 2005
Accepted August 2007

stakeholders use these benchmarks to evaluate firms' performance. They suggest that at least *some* firm stakeholders rely on heuristics, such as earnings benchmarks, to reduce information-processing costs when assessing firms' performance. In addition, prospect theory predicts that decision makers are less sensitive to *gains* but more sensitive to *losses* relative to a reference point (Kahneman and Tversky 1979). Firms, therefore, have incentives to beat earnings benchmarks to avoid the disproportionate adverse reactions to missing the benchmarks. Indeed, former SEC Chairman Arthur Levitt lamented how "unforgiving" the stock market is to firms that missed analysts' earnings forecasts (Levitt 1998).

Prior research finds that firms that beat (miss) earnings benchmarks have higher (lower) equity valuations after controlling for the magnitude of earnings performance (Barth et al. 1999; Bartov et al. 2002; Brown and Caylor 2005; Kasznik and McNichols 2002; Lopez and Rees 2002; Skinner and Sloan 2002; Myers and Skinner 2002). In contrast to the evidence on the equity market effects and incentives associated with beating earnings benchmarks, there is little evidence regarding whether and how creditors use earnings benchmarks in evaluating firm performance. Holthausen and Watts (2001, 26) point out that "it is not apparent that the relevance of a given number would be the same for equity investors and lenders," and call for more research into how accounting information is utilized by financial statement users other than equity investors.¹ This study answers this call by investigating whether and under what conditions beating earnings benchmarks reduces a firm's cost of debt.

Investigating the effects of earnings benchmarks within the context of the bond market is important for at least three reasons. First, firms are increasingly relying on debt financing. For example, in 2004, corporate bond issuances exceeded \$829.5 billion, whereas firms issued less than \$130 billion in equity (Thomson Financial 2004). Accordingly, small changes in bond prices represent large changes in capital allocation, so factors that influence bond prices are of great economic importance.

Second, the effect of beating earnings benchmarks is likely to be different in the bond market than in the equity market because of the rich information environment specific to the bond market. Approximately 95 percent of bondholders consist of institutional investors who are arguably more sophisticated and have access to more firm-specific information than other investors.² Credit-rating agencies have access to firms' nonpublic information such as minutes of board meetings, profit breakdown by product, budgets, forecasts, and internal capital allocation data (Ederington and Yawitz 1987; SEC 2003). Given their access to potentially more informative data regarding a firm's prospects, bond investors and rating agencies may place less reliance on earnings benchmarks in evaluating firm performance.

Third, even if bondholders and rating agencies use earnings benchmarks in evaluating firm performance, the relative importance of the three earnings benchmarks likely differs between the bond and equity markets. In the equity market, Brown and Caylor (2005) find that either beating analysts' earnings forecasts or reporting earnings increases leads to the highest three-day abnormal returns around the quarterly earnings announcement date after controlling for the level of unexpected earnings surprise. In contrast to equity investors,

¹ Similarly, Pettit et al. (2004, 1) argue that "credit ratings and rating agencies are mentioned only in passing in most business schools and remain one of the most *understudied* aspects of modern corporate finance" (emphasis in the original). This study enhances our understanding of how ratings agencies use accounting information.

² Prior research uses high institutional ownership to proxy for investor sophistication and richer firm information environments (e.g., Ayers and Freeman 2003; Bartov et al. 2000; Battalio and Mendenhall 2005; Collins et al. 2003; Hand 1990; Jiambalvo et al. 2002; Walther 1997).

bondholders have a fixed claim against the firm's value. They bear the firm's downside risk but do not share in the firm's upside growth potential (Fischer and Verrecchia 1997; Plummer and Tse 1999). Among the three earnings benchmarks, the most salient benchmark in assuring bondholders that the firm will survive and satisfy its financial obligations is reporting positive income. Indeed, prior research (Begley and Freeman 2002; Beatty et al. 2007) finds that profits and losses are treated asymmetrically in debt contracts, which suggests that creditors may focus on this particular benchmark. In contrast, beating last year's earnings indicates earnings growth, whereas beating analysts' forecasts suggests superior performance relative to analysts' expectations. *Ceteris paribus*, these latter two benchmarks may be less informative to bond investors regarding whether a firm will survive and satisfy its financial obligations.

To address whether beating earnings benchmarks affects a firm's cost of debt, I construct annual earnings benchmarks based on earnings per share, changes in earnings per share, and the single most recent analyst's forecasted earnings per share. I use firm credit ratings (Ahmed et al. 2002; Minton and Schrand 1999; Francis et al. 2005) and initial bond yield spread (Sengupta 1998; Shi 2003) to proxy for a firm's cost of debt. I examine the relation between credit-rating changes and yield spread and three separate dichotomous variables that denote whether firms beat the three annual earnings benchmarks. I control for the magnitudes of earnings, earnings changes, and earnings surprises along with other factors that prior research suggests influence firm credit ratings and yield spread. Including these controls allows me to assess the cost of debt effects specific to beating earnings benchmarks.

Consistent with bond investors using earnings benchmarks to evaluate firm performance, I find that beating earnings benchmarks is associated with a lower cost of debt (i.e., higher probability of a rating upgrade and smaller yield spread). I also find that the effects of beating earnings benchmarks are much more pronounced for firms with high default risk. This finding suggests that the importance of earnings benchmarks increases for firms with greater uncertainty about bondholders' future payoffs. In addition, I find that the reduction in a firm's cost of debt associated with reporting profits (i.e., beating the "profit" benchmark) generally equals or exceeds the effects associated with reporting an earnings increase and beating analysts' forecasts. Sensitivity analyses indicate that these results are robust to controlling for future earnings performance, which suggest that the lower cost of debt is not attributed to benchmark beating firms having superior future earnings performance.

For firms that likely beat earnings benchmarks through earnings management, the cost of debt effects may be attenuated. To test this possibility, I construct four alternative proxies of earnings management. They include (1) abnormal accruals from a forward-looking model (Dechow et al. 2003), (2) abnormal accruals from the modified Jones model, (3) abnormal cash flows from operations (Roychowdhury 2006), and (4) the unexpected change in effective tax rates (Dhaliwal et al. 2004). I then analyze whether the cost of debt effects of beating each benchmark are lower for firms with higher values of these four earnings management proxies. I find that the reduction in the cost of debt proxied by credit ratings is attenuated for those firms that likely have used earnings management to beat earnings benchmarks. Nonetheless, in most cases these firms still enjoy an increased likelihood of ratings upgrades, albeit smaller than other benchmark beating firms.

This study makes three contributions to the literature. First, this study identifies a new economically significant incentive for firms to beat three earnings benchmarks—to reduce their cost of debt. Results suggest that beating the profit benchmark boosts the likelihood

of a ratings upgrade by 110 percent (from 6 percent to 13 percent), decreases the likelihood of a ratings downgrade by 50 percent (from 18 percent to 9 percent), and reduces a firm's initial bond yield spread by 28 basis points.

Second, this study shows that the importance of beating the various earnings benchmarks is different in the debt market than has previously been established for the equity market. In contrast to the equity market where firms generally receive the greatest benefit from beating analysts' forecasts (Brown and Caylor 2006), I find that beating the profit benchmark usually generates the largest reduction in cost of debt. This difference between the debt and equity markets rationally reflects differences in payoffs across the two investments, as creditors share in a firm's downside risk but have limited ability to participate in upside potential. Among other implications, this evidence suggests that debt holders use a similar information set (i.e., earning benchmarks) as equity investors but that debt holders place differing weights on accounting information than do equity investors (i.e., debtholders use a different decision model).

Third, this study provides the first systematic evidence of a discontinuous relation between credit ratings (bond spread) and earnings information around the three benchmarks. Further analyses suggest that the discontinuity varies by benchmarks, firms' default risk, and method utilized to beat the benchmark. These findings are important for future research in modeling the determinants of credit ratings and bond spread. They also enhance our understanding of how debt investors use accounting information. Results suggest that debt investors use earnings benchmarks to assess firms' performance despite their potential information advantage.

The paper proceeds as follows. Section II develops testable hypotheses. Section III describes the research design. Section IV reports the sample selection and Section V presents the results. Section VI concludes.

II. HYPOTHESES DEVELOPMENT

Burgstahler and Dichev (1997) and Degeorge et al. (1999) suggest that firms' stakeholders, such as board of directors, equity investors, and creditors use earnings benchmarks as reference points or heuristics to evaluate firms' performance. Consistent with this conjecture, prior research finds that the stock market rewards firms for beatings the three earnings benchmarks, which cannot be completely explained by risk factors or future performance. For example, Barth et al. (1999) find that firms that consistently exceed previous years' earnings have higher price-earnings multiples after controlling for growth and risk, and the equity market rewards vanish if the pattern is broken. Brown and Caylor (2005) find positive abnormal returns around quarterly earnings announcements for firms reporting a profit, earnings increase, or beating analysts' earnings forecasts even after controlling for the level of unexpected earnings surprise. Similarly, Bartov et al. (2002), Kasznik and McNichols (2002), and Lopez and Rees (2002) find that firms meeting or beating analysts' earnings forecasts have higher abnormal returns and higher earnings response coefficients than firms missing analysts' forecasts. Kasznik and McNichols (2002, 757) suggest the unexplained abnormal returns "may instead reflect *investors' perceptions* that firms that consistently meet expectations are less risky than those that do not, and thus have a lower cost of equity capital" (emphasis added). In addition, Matsunaga and Park (2001) find that CEOs' cash bonuses are significantly lower when firms miss analysts' forecasts or experience earnings decreases after controlling for the general pay-for-performance relation.

This study investigates whether beating earnings benchmarks reduces a firm's cost of debt.³ Consistent with the findings in the equity market (Ball and Brown 1968; Beaver 1968), prior research finds that earnings information is useful in the bond market. For example, Ziebart and Reiter (1992) find that high return on assets (ROA) is associated with low bond yields and high bond ratings. Khurana and Raman (2003) find that fundamental scores that they construct to predict future earnings can explain cross-sectional differences in initial bond yields. Datta and Dhillon (1993) find that *quarterly* earnings surprises are positively associated with daily bond returns. Similarly, Plummer and Tse (1999) find that *annual* earnings changes are positively associated with annual bond returns. The similarity of earnings usefulness between the bond and equity markets suggests that the bond market may also reward firms for beating earnings benchmarks.

On the other hand, if the stock market rewards firms for beating earnings benchmarks in a way that is not completely rational, it is not obvious that similar results will hold in the bond market given the differences between the two markets. In particular, unlike the stock market, the bond market is dominated by institutional investors and credit-rating agencies. Ninety-five percent of bondholders are institutional investors, who may have an information advantage to equity investors and are arguably more sophisticated. Thus, bond investors may be less likely to rely on earnings benchmarks to evaluate firm performance. Rating agencies have access to firms' nonpublic information such as minutes of board meetings, profit breakdown by product, and internal capital allocation data (Ederington and Yawitz 1987; SEC 2003). With access to potentially more informative data regarding a firm's prospects, rating agencies may not rely on simple earnings benchmarks to assign credit ratings, a key determinant of bond yield. Accordingly, beating earnings benchmarks may not affect ratings and bond yield spreads.

Other evidence, however, questions the information advantage of bond investors and ratings agencies relative to equity investors.⁴ Bhojraj and Swaminathan (2004) argue that bond investors may be more passive than equity investors and, thus, less attentive to firm-specific information. Indeed, they find that the bond market misprices accruals just as the equity market does (e.g., Sloan 1996). Holthausen and Leftwich (1986) find that rating changes tend to occur *after* abnormal stock returns. Thus, it is an empirical question whether bondholders and ratings agencies have an information advantage and likewise, whether they utilize earnings benchmarks in pricing bonds and assigning ratings.

Because bondholders' claim on a firm's income is fixed, earnings are less useful for bondholders of firms with low default risk. Consistent with this proposition, prior research finds cross-sectional differences in the usefulness of earnings to bondholders as a function of a firm's default risk. For example, Plummer and Tse (1999) find that earnings changes become less closely associated with bond returns as bond ratings improve. To the extent that beating earnings benchmarks lowers the cost of debt, I expect that such effects will increase with firms' default risk. Specifically, I hypothesize that beating earnings benchmarks is associated with a lower cost of debt, and the association is more pronounced for high default risk firms than for low default risk firms. In alternative form, my first two hypotheses are:

H1: *Ceteris paribus*, beating earnings benchmarks lowers a firm's cost of debt.

³ Burgstahler (1997) finds that the likelihood of S&P's equity and credit ratings upgrades increases in the vicinity of zero earnings changes and zero earnings. However, he does not perform statistical tests or control for other factors such as firm size that explain debt ratings.

⁴ Institutional investors' information advantage is also challenged in the equity market. For example, Ali et al. (2000) find that high institutional ownership *exacerbates* rather than mitigates the mispricing of accruals.

H2: Beating earnings benchmarks has more pronounced effects on the cost of debt for firms with high default risk than those with low default risk.

Focusing on the equity market consequences, Brown and Caylor (2005) find that either reporting quarterly earnings increases or beating analysts' forecasts receives the largest equity market reward (i.e., three days abnormal returns around earnings announcement), while reporting profits receives the least reward after controlling for the level of unexpected earnings surprise. Frank and Rego (2004, 5) even question the importance of positive net income and historical annual earnings as targets for firms with analysts following.

I anticipate that the relative benefits of beating each earnings benchmark differ between the bond market and the equity market. Bondholders have a fixed claim against a firm's value. They bear the firm's downside risk but do not share in the firm's upside growth potential (Fischer and Verrecchia 1997; Plummer and Tse 1999). Among the three earnings benchmarks, reporting profits is likely the most salient benchmark in assuring bondholders that the firm will survive and satisfy its financial obligations. Prior research finds that there is an asymmetric treatment of profits and losses in debt contracts. Begley and Freeman (2002) find that in public debt covenants the dividend pools typically include only 50 percent of net profit but deduct 100 percent of net losses. Beatty et al. (2007) find that for firms with net worth covenants, reporting a profit only partially increases the covenant slack, whereas reporting a loss reduces the slack by 100 percent. Similarly, in a case study of L.A. Gear, DeAngelo et al. (2002) find that most of L.A. Gear's bank credit agreements have minimum earnings requirements, such as requiring L.A. Gear to report positive quarterly earnings.⁵ In contrast, beating last year's earnings indicates earnings growth, whereas beating analysts' forecasts suggests superior performance relative to analysts' expectations. *Ceteris paribus*, these latter two benchmarks may be less informative to bond investors regarding whether a firm will survive and satisfy its financial obligations.. This leads to my third hypothesis (in alternative form):

H3: Reporting a profit has a more pronounced effect on a firm's cost of debt than reporting an earnings increase or beating analysts' earnings forecasts.

III. RESEARCH DESIGN

To investigate whether firms beating earnings benchmarks have a lower cost of debt, I use firm credit ratings and initial bond yield spread to proxy for a firm's cost of debt. Prior research has used both credit ratings and initial bond yield spread to proxy for cost of debt (Ahmed et al. 2002; Minton and Schrand 1999; Francis et al. 2005; Sengupta 1998; Shi 2003). Credit ratings represent the rating agencies' assessment of a firm's credit worthiness and can affect a firm's access to bank loans, bonds, and commercial paper markets.⁶ Research suggests that downward rating changes affect both bond and stock prices (Dichev and Piotroski 2001; Hand et al. 1992; Holthausen and Leftwich 1986) and can trigger

⁵ DeAngelo et al. (2002, 32) explain why "debt contracts typically constrain accounting earnings and/or net worth and not, as one might suppose *ex ante*, operating (or free) cash flow." They note that L.A. Gear had consecutive accounting losses from 1991 to 1996 but positive operating cash flows in half of these years by liquidating its working capital. Just one year prior to bankruptcy, L.A. Gear had a cash balance of almost 34 percent of its total assets. DeAngelo et al. (2002) conclude that accounting-based debt covenants are stronger disciplinary mechanisms than periodic interest payments to debtholders.

⁶ Asquith et al. (2005, 107) report that debt ratings are the second most frequently used measure in bank loan contracts that have performance pricing measures.

accelerated payment of existing debt (Reason 2002). Initial bond yield spread (i.e., the corporate bond yields at the issuance date minus the Treasury bond yields with comparable maturity) represents the risk premium that firms must pay to borrow money in the bond market and is a direct measure of a firm's incremental cost of debt (Sengupta 1998; Shi 2003).

I use the following two models to test if beating earnings benchmarks affect credit ratings and initial bond yields:

$$\begin{aligned}\Delta Rating_{it+1} = & \alpha_0 + \alpha_1 Benchmark_{it} + \alpha_2 EarningsControl_{it} + \beta_1 \Delta CFO_{it} \\ & + \beta_2 \Delta StdRoa_{it} + \beta_3 \Delta Times_{it} + \beta_4 \Delta RND_{it} + \beta_5 \Delta StdRet_{it} \\ & + \beta_6 \Delta BM_{it} + \beta_7 \Delta Size_{it} + \beta_8 \Delta Lev_{it} + \sum_i \beta_i Year_{it} + \varepsilon_{it}\end{aligned}\quad (1)$$

$$\begin{aligned}Spread_{it+1} = & \alpha_0 + \alpha_1 Benchmark_{it} + \alpha_2 EarningsControl_{it} + \beta_1 Rating_{it} \\ & + \beta_2 CFO_{it} + \beta_3 StdRoa_{it} + \beta_4 Times_{it} + \beta_5 RND_{it} + \beta_6 StdRet_{it} \\ & + \beta_7 BM_{it} + \beta_8 Size_{it} + \beta_9 Lev_{it} + \beta_{10} BC_{it} \\ & + \beta_{11} Senior_{it} + \beta_{12} IssueSize_{it} + \beta_{13} Call_{it} + \varepsilon_{it}\end{aligned}\quad (2)$$

where:

$\Delta Rating_{it+1} = Rating_{it+1} - Rating_{it}$, where $Rating_{it}$ is firm i 's Standard & Poor's senior debt rating in year t . Standard & Poor's rates a firm's debt from AAA (indicating a strong capacity to pay interest and repay principal) to D (indicating actual default). I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Thus a negative $\Delta Rating_{it+1}$ corresponds to a rating upgrade and a positive $\Delta Rating_{it+1}$ corresponds to a rating downgrade. Appendix A provides a complete conversion table between ratings letters and ratings number;

$Spread_{it+1}$ = the yield to maturity at the issuance date for the largest bond that firm i issued in year $t+1$, minus the Treasury bond yield with similar maturity. I measure $Spread_{it+1}$ as a percentage;

$Benchmark_{it}$ takes one of the three following three specifications:⁷

$Profit_{it} = 1$ if firm i 's basic earnings per share before extraordinary items is greater than or equal to 0 in year t , and 0 otherwise;

$Incr_{it} = 1$ if firm i 's earnings per share before extraordinary items in year t is greater than or equal to that of year $t-1$, and 0 otherwise;

$Surp_{it} = 1$ if firm i 's earnings per share beats or meets the most recent analyst's forecast in year t , and 0 otherwise;

$EarningsControl_{it}$ takes one of the following continuous earnings variables corresponding to each earnings benchmark:

⁷ When I define $Benchmark_{it}$ excluding "meeting" observations (i.e., those in which performance equals the benchmark), inferences remain the same.

- EPS_{it} = firm i 's earnings per share before extraordinary items in year t divided by its stock price at the end of year $t-1$, corresponding to the profit benchmark ($Profit_{it}$);
- ΔEPS_{it} = changes in firm i 's earnings per share before extraordinary items between year t and $t-1$, divided by its stock price at the end of year $t-1$, corresponding to the earnings increase benchmark ($Incr_{it}$); and
- UE_EPS_{it} = firm i 's actual earnings per share minus the single most recent analyst's forecast for year t , divided by its stock price at the end of year $t-1$, corresponding to analysts' earnings forecast benchmark ($Surp_{it}$).

Other control variables include:

- CFO_{it} = firm i 's operating cash flows at year t deflated by total assets at the beginning year, following Francis et al. (2005, 302);
- $StdRoa_{it}$ = firm i 's standard deviation of ROA calculated using five years data from year $t-4$ to t . ROA is net income before extraordinary items deflated by total assets at the beginning year;
- $Times_{it}$ = the natural log of $(1 + \text{times-to-interests-earned ratio})$, where times-to-interests-earned ratio is firm i 's operating income before depreciation and interest expense divided by interest expense both at year t ; ⁸
- RND_{it} = firm i 's research and development expense in year t deflated by total assets at the beginning year;
- $StdRet_{it}$ = the standard deviation of firm i 's daily stock returns during year t ;
- BM_{it} = the natural log of firm i 's book value of equity divided by its market value of equity, both measured at the end of year t ;
- $Size_{it}$ = the natural log of firm i 's total assets at the end of year t ;
- Lev_{it} = firm i 's long-term debt divided by total assets at the end of year t ; and
- $Year_{it}$ = 1 if observation i is in year t , and 0 otherwise.

I define each change variable in model (1) as the first difference of the above variables, such as $\Delta CFO_{it} = CFO_{it} - CFO_{it-1}$. Additional control variables in model (2) include:

- $Rating_{it}$ = in model (2), $Rating_{it}$ is bond i 's Standard & Poor's rating at the issue date. I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Appendix A provides a complete conversion table;
- BC_{it} = the difference between the average yield on Moody's Aaa bonds and the average yield of ten-year U.S. Treasury bonds for the issue month;
- $Senior_{it}$ = 1 for senior bonds and 0 for subordinated bonds;
- $IssueSize_{it}$ = the natural log of the offering amount of the bond (in millions of dollars); and
- $Call_{it}$ = the ratio of the number of years to first call divided by the number of years to maturity. $Call_{it}$ takes the value of 1 if there is no call provision and 0 if it is callable from the date of issuance.

⁸ I define $Times_{it}$ as the natural log of $(1 + \text{times-to-interests-earned ratio})$ to account for the possible nonlinear relation between this ratio and the cost of debt.

I use a “rating change” model to test if beating earnings benchmarks increases a firm’s probability of ratings upgrades. As outsiders, we do not know the exact metrics that ratings agencies use and what weight they put on these metrics in assigning ratings. Thus, most empirical research on the determinants of credit ratings suffers from potential correlated omitted variable concerns. In addition, credit ratings are “sticky,” which implies that any correlated omitted variables or the error terms from a ratings-level regression are likely correlated over time. A change specification mitigates the effects of correlated omitted variables and autocorrelation in the error terms. I measure the first difference of credit ratings as $\Delta Rating_{it+1} = Rating_{it+1} - Rating_{it}$.⁹ In the initial bond yield spread analysis, in addition to firm-specific and macroeconomic variables, I include bond-specific variables, such as bond rating and contract features, which should lessen concerns regarding correlated omitted variables. I measure $Spread_{it+1}$ using new bonds issued within 360 days after year t ’s earnings announcement.

I construct the earnings benchmark dichotomous variables using earnings per share, changes in earnings per share, and the single most recent analyst’s forecasted earnings per share.¹⁰ To test H1, I estimate model (1) and (2) including one benchmark specification ($Profit_{it}$, $Incr_{it}$, and $Surp_{it}$) at a time. For each benchmark, I add the corresponding continuous earnings variable to control for the “normal” effect of earnings or profitability on the cost of debt. For example, I control for EPS_{it} when testing the $Profit_{it}$ benchmark. This control is important to dissuade concerns that the earnings benchmark coefficients are simply attributable to the general association between firm performance and the cost of debt. Accordingly, the coefficient for each benchmark indicator represents the average effect of beating the earnings benchmark on the cost of debt incremental to firm performance and other determinants of cost of debt. If beating earnings benchmarks is associated with a lower cost of debt, then I expect the coefficients of the benchmarks to be negative in both models (1) and (2). In model (1), negative coefficients on earnings benchmarks indicate that beating earnings benchmarks increases (decreases) the probability of ratings upgrade (downgrade). In model (2), negative coefficients on earnings benchmarks indicate that beating earnings benchmarks decreases the risk premium that bond investors require.

To test H2 (i.e., whether the impact of beating an earnings benchmark is more pronounced for firms with high default risk), I partition the sample into high and low default risk subsamples. I first test if beating earnings benchmarks is associated with lower cost of debt in both groups and then test whether the benchmarks coefficients are different between the two groups. If the effects of beating earnings benchmarks are more pronounced for high default risk firms than for low default risk firms, then I expect: (1) the effects of beating earnings benchmarks only exist in the high default risk group, or (2) the effects of beating earnings benchmarks exist in both groups but are larger in magnitude (i.e., more negative) for high default risk firms than for low default risk firms.

⁹ I use Compustat’s annual files to identify S&P’s credit ratings, which are valid at the fiscal year end. Thus, $\Delta Rating_{it+1}$ reflects ratings changes that occur between the end of fiscal year t and $t+1$. In sensitivity tests, I exclude a small number of observations (about 1 percent of all ratings changes) that change rating after year t ’s end date but at least 30 days before year t ’s earnings announcement date. Inferences remain the same.

¹⁰ I use earnings per share instead of earnings to capture firms that beat earnings benchmarks by manipulating shares outstanding (Bens et al. 2003; Myers and Skinner 2002). I use the single most recent analyst’s forecast from the I/B/E/S detail history file instead of the consensus forecast from the I/B/E/S summary file to mitigate the effects of stale analysts’ forecasts (Brown 1991; Brown and Kim 1991; Ayers et al. 2006). Inferences are the same using the adjusted or unadjusted I/B/E/S file following Baber and Kang (2002) and Payne and Thomas (2003).

To test H3, I estimate models (1) and (2) including all three benchmarks and the three continuous earnings control variables to assess the relative importance and the corresponding incentives associated with beating each benchmark for both the aggregate sample and the high default risk subsample. The coefficient for each benchmark indicator represents the incremental effect of meeting or beating that benchmark after controlling for the effects of the other two benchmarks. If reporting profits yields the largest reduction in firms' cost of debt, then I expect the coefficient of $Profit_{it}$ to be significantly more negative than the coefficients for $Incr_{it}$ and $Surp_{it}$.

Prior research finds that firms with better performance and less risk have lower cost of debt (Ahmed et al. 2002; Campbell and Taksler 2003; Kaplan and Urwitz 1979; Sengupta 1998; Shi 2003). I include operating cash flows (CFO_{it}), times-to-interests-earned ratio ($Times_{it}$), and book-to-market ratio (BM_{it}) as additional controls for firm performance. I include R&D expenditure (RND_{it}), the dispersion of ROA ($StdRoa_{it}$),¹¹ the dispersion of equity returns ($StdRet_{it}$), size ($Size_{it}$), and leverage (Lev_{it}) to control for firm risk. Since the dependent variable in model (1) is the change of credit ratings, I measure each of these variables in changes for model (1).¹² I expect the performance and risk variables to be positively associated with cost of debt except for operating cash flows, times-to-interests-earned ratio, and size. To control for time varying factors related to ratings, I include year indicators ($Year_{it}$) in model (1) following Blume et al. (1998). Following Sengupta (1998) and Shi (2003), I calculate BC_{it} (*Business Cycle*) as the yield difference between Moody's Aaa bonds and ten-year Treasury bonds for the issue month to control for the time-series variation in risk premium over the business cycle. I expect this risk premium to be positively associated with the new bond yield spread.

Prior literature documents that bond characteristics also affect the initial bond yield spread (Bhojraj and Sengupta 2003; Fisher 1959; Khurana and Raman 2003; Sengupta 1998; Shi 2003; Ziebart and Reiter 1992). I include the bond's credit rating ($Rating_{it}$), seniority status ($Senior_{it}$), issue size ($IssueSize_{it}$), and call provisions ($Call_{it}$) to control for bond features associated with the yield spread. I transform the Standard & Poor's bond ratings into integers such that higher rating numbers indicate higher default risk. I expect a positive relation between $Rating_{it}$ and yield spread. Holders of senior bonds have more protection than subordinated bondholders if bond issuers default, so I expect senior bonds to have a smaller yield spread. $IssueSize_{it}$ reflects a bond's liquidity (i.e., larger size, higher liquidity), which is generally negatively associated with yield spread (Sengupta 1998). On the other hand, $IssueSize_{it}$ also reflects a firm's overall debt burden, which is generally positively associated with bond yield spread (Shi 2003). Thus, I have no prediction on $IssueSize_{it}$. A bond's call provision exposes bondholders to interest risk (i.e., a lower call ratio implies higher risk exposure for bondholders), so I expect $Call_{it}$ to be negatively associated with yield spread.

IV. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

I use the I/B/E/S detail history file to construct the earnings surprise indicator ($Surp_{it}$) and the magnitude of earnings surprises (UE_EPS_{it}). I calculate all other firm-specific variables using data from Compustat and CRSP. I exclude public utilities (two-digit SIC code 49) and financial service firms (two-digit SIC codes between 60 and 67) because these

¹¹ Inferences remain the same if I add the dispersion of cash flows in addition to the dispersion of earnings in models (1) and (2). The dispersion of cash flow and the dispersion of earnings are highly correlated in the sample ($\rho = 0.48$, Spearman and Pearson correlations).

¹² Because I include the three continuous earnings variables to control for the underlying performance constructs associated with each earnings benchmark, I do not calculate these variables as "change" variables.

industries have different operating characteristics and different debt financing activities than industrial firms.¹³ I winsorize all continuous variables at the top and bottom 1 percent to mitigate the influence of extreme observations.

The Credit Ratings Sample

Standard & Poor's (S&P) usually assigns each company a long-term "issuer" rating intended to measure a company's ability to meet its senior obligations as well as specific ratings for each debt issuance according to the debt contract. Senior debt ratings are usually the same as the issuer rating. I collect firms' senior debt ratings from the annual Compustat file (data item 280) available between 1985 and 2003. I classify firms into low and high default risk subsamples based on whether they have investment grade rating (BBB– or above) or non-investment grade rating (BB+ or below). The final sample includes 8,878 firm-year observations from 1985 to 2002: 2,848 observations in the high default risk subsample and 6,030 observations in the low default risk subsample.¹⁴ Appendix A reports the distribution of observations across the ratings categories.¹⁵

Table 1 presents the distributions of the dependent variable, $\Delta Rating_{it+1}$, which takes values from –4 to 4, where a negative number indicates a rating upgrade and a positive number indicates a rating downgrade.¹⁶ Panel A shows that in the aggregate sample only 22 percent of firm-years change credit ratings annually, with ratings downgrades more prevalent than upgrades (14 percent versus 8 percent). In the high default risk sample, the percentage of firms experience rating changes is a little bit higher (i.e., 27 percent) and the proportion of ratings downgrades is similar to ratings upgrades (14 percent versus 13 percent).

Panels B and C of Table 1 report the directions of ratings changes for firms missing/ beating earnings benchmarks for the aggregate and the high default risk samples, respectively. If beating earnings benchmarks is associated with ratings upgrades, then we should observe an increase in the proportion of firms that beat a benchmark as ratings changes move from downgrades to unchanged and to upgrades. Indeed, this is the case for all three benchmarks. Panel B indicates that the percentage of firms reporting profits increases from 67 percent among firms experiencing ratings downgrades to 86 percent among firms with no ratings changes, and then to 90 percent among firms experiencing ratings upgrades. Panel C shows that for all benchmarks in the high default risk sample, firms that experience ratings downgrades are more likely to miss rather than beat a benchmark. In particular, firms that experience a ratings downgrade are 1.8 times more likely to miss rather than beat the profit benchmark (64 percent versus 36 percent), twice more likely to miss rather than beat the earnings increase benchmark (68 percent versus 32 percent) and 1.2 times more likely to miss rather than beat analysts' earnings forecasts (55 percent versus 45 percent).

Table 2 presents descriptive statistics for all independent variables. For the aggregate sample, approximately 84 percent of firm-years report profits, whereas 61 percent report

¹³ For example, Smith (1986) points out that utility firms access the debt market more extensively than industrial firms.

¹⁴ I exclude firms that change ratings dramatically in adjacent years (i.e., $|Rating_{it+1} - Rating_{it}| > 4$) due to coding errors or significant events such as a merger or acquisition. I exclude such observations because model (1) does not account for such factors.

¹⁵ Inferences are robust to excluding observations that are rated CCC– or below for both models (1) and (2).

¹⁶ Inferences are identical if I separate ratings changes into three major categories: ratings upgrades, rating downgrades and ratings unchanged.

TABLE 1
Descriptive Statistics of Credit Ratings Changes

Panel A: Distributions of Credit Ratings Changes^a

Credit Ratings Changes	Aggregate Sample		High Default Risk Subsample		Low Default Risk Subsample	
	Observations	Percentage	Observations	Percentage	Observations	Percentage
Ratings Downgrade	1222	14%	412	14%	810	13%
Ratings Unchanged	6914	78%	2069	73%	4845	80%
Ratings Upgrade	742	8%	367	13%	375	6%
Total	8878	100%	2848	100%	6030	100%

Panel B: Ratings Changes versus Miss/Beat Benchmarks for the Aggregate Sample^b

Frequency Percentage	Reporting Losses	Reporting Profits	Reporting Earnings Decreases	Reporting Earnings Increases	Miss Analysts' Earnings Forecast	Beat Analysts' Earnings Forecast
Ratings Downgrade	406 33%	816 67%	722 59%	500 41%	566 46%	656 54%
Ratings Unchanged	980 14%	5934 86%	2606 38%	4308 62%	2570 37%	4344 63%
Ratings Upgrade	73 10%	669 90%	139 19%	603 81%	243 33%	499 67%

Panel C: Ratings Changes versus Miss/Beat Benchmarks for the High Default Risk Sample^c

Frequency Percentage	Reporting Losses	Reporting Profits	Reporting Earnings Decreases	Reporting Earnings Increases	Miss Analysts' Earnings Forecast	Beat Analysts' Earnings Forecast
Ratings Downgrade	262 64%	150 36%	281 68%	131 32%	226 55%	186 45%
Ratings Unchanged	650 31%	1419 69%	884 43%	1185 57%	846 41%	1223 59%
Ratings Upgrade	59 16%	308 84%	77 21%	290 79%	129 35%	238 65%

^a Ratings Downgrade includes all firm-years whose one-year-ahead rating number becomes bigger. Ratings Unchanged includes all firm-years whose one-year-ahead rating is the same as current rating. Ratings Upgrade includes all firm-years whose one-year-ahead rating number becomes smaller. The high default risk subsample includes observations that are rated as non-investment grade (i.e., ratings are BB+ or below). The low default risk subsample includes observations that are rated as investment grade (i.e., ratings are BBB- or above).

^b The three tables in this panel show the cross-distributions between ratings changes and meet or beat the three earnings benchmarks for 8,878 observations in the aggregate credit rating sample. Ratings Downgrade includes 1,222 observations whose one-year-ahead rating number becomes bigger. Ratings Unchanged includes 6,914 observations whose one-year-ahead rating remains unchanged. Ratings Upgrade includes 742 observations whose one-year-ahead rating number becomes smaller.

^c The three tables within this table show the cross-distributions between ratings changes and meet or beat the three earnings benchmarks for 2,848 observations that are rated as non-investment grade. Ratings Downgrade includes 412 observations whose one-year-ahead rating number becomes bigger. Ratings Unchanged includes 2,069 observations whose one-year-ahead rating remains unchanged. Ratings Upgrade includes 367 observations whose one-year-ahead rating number becomes smaller.

The percentage total is not always equal to the sum of individual percentage amounts due to rounding.

TABLE 2
Descriptive Statistics for Credit Ratings Sample^a

<u>Variable</u>	<u>Significant Difference^b</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>Lower Quartile</u>	<u>Upper Quartile</u>
<i>ΔRating</i>						
Aggregate		0.103	0	0.755	0	0
High Default	HD < LD	0.044	0	0.865	0	0
Low Default		0.130	0	0.696	0	0
<i>Profit</i>						
Aggregate		0.836	1	0.371	1	1
High Default	HD < LD	0.659	1	0.474	0	1
Low Default		0.919	1	0.273	1	1
<i>Incr</i>						
Aggregate		0.609	1	0.488	0	1
High Default	HD < LD	0.564	1	0.496	0	1
Low Default		0.631	1	0.483	0	1
<i>Surp</i>						
Aggregate		0.619	1	0.486	0	1
High Default	HD < LD	0.578	1	0.494	0	1
Low Default		0.639	1	0.48	0	1
<i>EPS</i>						
Aggregate		0.035	0.055	0.137	0.023	0.081
High Default	HD < LD	-0.01	0.039	0.222	-0.042	0.087
Low Default		0.056	0.058	0.055	0.036	0.08
<i>ΔEPS</i>						
Aggregate		0.007	0.006	0.165	-0.02	0.025
High Default	HD > LD	0.014	0.008	0.275	-0.052	0.058
Low Default		0.003	0.006	0.064	-0.012	0.019
<i>UE_EPS</i>						
Aggregate		-0.003	0	0.036	-0.002	0.003
High Default	HD ≈ LD	-0.008	0	0.057	-0.006	0.005
Low Default		-0.001	0	0.019	-0.001	0.002
<i>ΔCFO</i>						
Aggregate		-0.001	-0.001	0.108	-0.045	0.042
High Default	HD ≈ LD	0.001	0.001	0.139	-0.063	0.061
Low Default		-0.002	-0.001	0.09	-0.039	0.036
<i>ΔStdRoa</i>						
Aggregate		-0.001	0	0.023	-0.004	0.004
High Default	HD ≈ LD	-0.002	0	0.033	-0.007	0.007
Low Default		0	0	0.016	-0.003	0.004
<i>ΔTimes</i>						
Aggregate		-0.027	0.012	0.489	-0.181	0.184
High Default	HD ≈ LD	-0.059	0.003	0.633	-0.249	0.213
Low Default		-0.013	0.016	0.403	-0.16	0.171
<i>ΔRND</i>						
Aggregate		-0.001	0	0.018	0	0
High Default	HD ≈ LD	-0.002	0	0.025	0	0
Low Default		0	0	0.014	0	0
<i>ΔStdRet</i>						
Aggregate		0.001	0	0.007	-0.003	0.004
High Default	HD ≈ LD	0.001	0	0.01	-0.005	0.006
Low Default		0	0	0.006	-0.003	0.004

(continued on next page)

TABLE 2 (continued)

<u>Variable</u>	<u>Significant Difference^b</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>Lower Quartile</u>	<u>Upper Quartile</u>
ΔBM						
Aggregate	HD > LD	0.022	0.004	0.429	−0.204	0.231
High Default		0.066	0.059	0.569	−0.252	0.369
Low Default		0	−0.01	0.341	−0.191	0.181
$\Delta Size$						
Aggregate	HD ≈ LD	0.094	0.062	0.211	−0.008	0.154
High Default		0.094	0.047	0.264	−0.041	0.178
Low Default		0.094	0.066	0.181	0.006	0.147
ΔLev						
Aggregate	HD ≈ LD	0.006	−0.003	0.077	−0.029	0.032
High Default		0.007	−0.002	0.101	−0.041	0.047
Low Default		0.005	−0.003	0.062	−0.025	0.028

^a The aggregate sample consists of 8,878 firm year observations from 1985 to 2002. The high default risk subsample consists of 2,848 firm year observations whose S&P senior debt ratings are non-investment grade (BB+ or below). The low default risk subsample consists of 6,030 firm year observations whose S&P senior debt ratings are investment grade (BBB- or above). Appendix B provides variable definitions.

^b Significant differences between the high default risk and low default risk subsamples are based on both the t-statistics of means and z-statistics of Wilcoxon Rank Sum Tests of medians ($p < 0.10$).

earnings increases and 62 percent beat the most recent analyst's earnings forecast. Univariate comparisons indicate that low default risk firms beat the three earnings benchmarks more frequently than do high default risk firms. Low default risk firms also have significantly higher earnings per share, smaller change in book-to-market ratio, and smaller earnings changes (ΔEPS_{it}) than do high default risk firms. All other independent variables are similar between the two subsamples.

Table 3 presents univariate correlations among all variables for the aggregate sample. I report Spearman correlations above the diagonal and Pearson correlations below the diagonal. To simplify the presentation, I only report correlations that are significant at 10 percent significance levels (two-tailed test). Consistent with H1, all three earnings benchmark indicators are negatively associated with $\Delta Rating_{it+1}$, indicating that beating any of the earnings benchmarks in year t is associated with a higher probability of ratings upgrades from year t to $t+1$. All control variables are significantly correlated with $\Delta Rating_{it+1}$ with the expected sign except for ΔRND_{it} .

The Initial Bond Yield Spread Sample

To gather the bond issue sample, I collect nonconvertible, fixed-rate bonds issued by U.S. firms from 1983 to 2002 from Securities Data Company's Global New Issues database. I exclude bonds with asset-backed or credit-enhancement features because the spreads of these bonds reflect the creditworthiness of the collateral rather than the creditworthiness of the firm (Campbell and Taksler 2003). For firms with multiple issuances in a given year, I only include the issue with the largest offering amount (Khurana and Raman 2003). Similar to the credit ratings sample, I partition the new bond issue sample into high default risk group if a bond has an investment grade rating (BBB- or above) and low default risk group if a bond has a non-investment grade rating (BB+ or below). The final sample includes 1,798 bond issues: 1,084 low default risk bonds issued by 464 firms and 250 high default risk bonds issued by 181 firms. Appendix A reports the distribution of observations across the ratings categories.

TABLE 3
Significant Correlations for the Aggregate Credit Ratings Sample

	$\Delta Rating$	$Profit$	$Incr$	$Surp$	EPS	ΔEPS	UE_EPS	ΔCFO	$\Delta StdRoa$	$\Delta Times$	ΔRND	$\Delta StdRet$	ΔBM	$\Delta Size$	ΔLev
$\Delta Rating$		-0.17	-0.20	-0.07	-0.20	-0.20	-0.08	-0.08	0.07	-0.23	-0.05	0.10	0.13	-0.11	0.15
$Profit$	-0.18		0.35	0.15	0.64	0.37	0.13	0.16	-0.20	0.23	0.02	-0.10		0.28	-0.14
$Incr$	-0.18	0.35		0.10	0.47	0.85	0.09	0.32	-0.20	0.44	0.03	-0.07	-0.10	0.16	-0.19
$Surp$	-0.08	0.15	0.10		0.14	0.11	0.84	0.04	-0.04	0.08		-0.02		0.06	-0.04
EPS	-0.16	0.61	0.32	0.13		0.53	0.18	0.19	-0.13	0.28	0.02	-0.12	-0.13	0.24	-0.17
ΔEPS	-0.12	0.26	0.41	0.06	0.46		0.14	0.40	-0.20	0.51	0.04	-0.09	-0.11	0.12	-0.22
UE_EPS	-0.10	0.22	0.11	0.35	0.31	0.14		0.06	-0.04	0.09		-0.03	-0.04	0.04	-0.05
ΔCFO	-0.07	0.13	0.25	0.02	0.16	0.31	0.03		-0.07	0.26	-0.04	-0.03	-0.05	-0.06	-0.17
$\Delta StdRoa$	0.06	-0.16	-0.13	-0.03	-0.16	-0.13	-0.05	-0.08		-0.13	0.09	0.06	-0.14	-0.03	0.04
$\Delta Times$	-0.17	0.25	0.35	0.07	0.20	0.31	0.08	0.23	-0.13			-0.09	-0.03	-0.04	-0.35
ΔRND			-0.03			-0.02		-0.11	0.05	0.05		-0.02	-0.03	0.10	
$\Delta StdRet$	0.10	-0.14	-0.08	-0.03	-0.16	-0.10	-0.08	-0.02	0.06	-0.09	0.00		0.10	0.02	0.04
ΔBM	0.13		-0.10		0.06	-0.03	0.03	-0.03		-0.12	-0.05	0.15		0.10	-0.04
$\Delta Size$	-0.07	0.20	0.10	0.07	0.21	0.04	0.08	-0.12	-0.03	-0.09	0.12	0.02	0.12		0.15
ΔLev	0.11	-0.09	-0.14		-0.10	-0.11	-0.02	-0.15		-0.33	0.06	0.02	-0.07	0.29	

The aggregate credit ratings sample consists of 8,878 firm year observations from 1985 to 2002. Spearman (Pearson) correlations are above (below) the diagonal. All correlations in the table are significant at $p < .10$ (two-tailed) significance levels. Appendix B provides variable definitions.

Table 4 presents descriptive statistics for the aggregate sample as well as subsamples partitioned by default risk. The mean yield spread equals 1.46 percent. As expected, the yield spread of high default risk bonds is much higher (3.69 percent) than that of low default risk bonds (1.10 percent). Approximately 92 percent of sample firms report profits, whereas 64 percent report earnings increases and 63 percent beat analyst's earnings forecasts for the aggregate sample. Compared to the credit ratings sample, the new bond issue sample has a larger proportion of firms beating all three earnings benchmarks, because better-performing firms are more likely to issue public bonds (Denis and Mihov 2003). Also as expected, low default risk firms are more likely to report profits, earnings increases, and positive earnings surprises.¹⁷ Univariate comparisons in Table 4 also indicate that low default risk firms have larger cash flows from operations, bigger times-to-interests-earned ratio, higher R&D expenditure, less volatile return on assets and stock returns, lower book-to-market ratio, larger size, and lower leverage than high default risk firms.

Table 5 presents Spearman (above the diagonal) and Pearson (below the diagonal) correlations among all variables for the aggregate new bond issue sample. Consistent with H1, all three earnings benchmark indicators are negatively associated with $Spread_{t+1}$, indicating that beating any of the earnings benchmarks in year t is associated with a smaller yield spread in year $t+1$.¹⁸ Consistent with H3, the Pearson correlation between $Profit_t$ and $Spread_{t+1}$ is stronger ($\rho = -0.26$) than the correlation between $Incr_t$ and $Spread_{t+1}$ ($\rho = -0.13$) and the correlation between $Surp_t$ and $Spread_{t+1}$ ($\rho = -0.05$). Pearson correlations indicate that all control variables are significantly correlated with $Spread_{t+1}$ with the expected sign except for RND_{it} .

V. RESULTS

I estimate model (1) using an ordered logit model and model (2) using ordinary least squares. I report p-values based on robust standard errors (White 1980) to control for heteroscedasticity. To control further for potential serial correlation in the credit ratings sample, I cluster the error term at the firm level (Rogers 1993; Wooldridge 2002).

The Credit Ratings Sample

Panel A of Table 6 presents the results of estimating model (1) for the credit ratings sample. The results in columns (1)–(3) suggest that consistent with H1, beating any of the three earnings benchmarks is associated with a higher probability of a ratings upgrade ($p \leq 0.001$, one-tailed test). Column (10) indicates that for the aggregate sample, beating any of the three earnings benchmarks is associated with a higher probability of a rating upgrade even after controlling for the effects of the other two benchmarks ($p \leq 0.003$, one-tailed test). Columns (4)–(9) test whether the effect of beating earnings benchmarks exists for both the high and low default risk samples. I find that except for the profit benchmark in the low default risk sample, beating any earnings benchmarks is associated with a higher probability of a ratings upgrade in both high and low default risk samples ($p \leq 0.019$, one-tailed test).¹⁹ Based on these coefficients estimates, Panel B tests whether the benchmark effects are significantly different between high and low default risk samples. The results

¹⁷ In the new bond issue sample, the default risk measure ($Rating_{it}$) is bond-specific. For senior bonds, the bond rating is usually the firm credit rating. In my sample, 92 percent of bonds are senior bonds. For simplicity, I refer to "high default risk firms" instead of "firms that issue high default risk bonds."

¹⁸ $Surp_{it}$ is only significantly associated with $Spread_{t+1}$ using Pearson correlation.

¹⁹ The insignificant result for the profit benchmark in the low default risk sample ($p = 0.14$, one-tailed test) may result from limited observations reporting a loss. Table 2 indicates that only 8 percent of low default risk firms report a loss.

TABLE 4
Descriptive Statistics for the Initial Bond Spread Sample^a

Variable	Significant Difference ^b	Mean	Median	Std. Dev.	Lower Quartile	Upper Quartile
Spread						
Aggregate	HD > LD	1.457	1.04	1.168	0.7	1.75
High Default		3.688	3.743	1.16	2.92	4.45
Low Default		1.097	0.94	0.66	0.65	1.38
Profit						
Aggregate	HD < LD	0.917	1	0.276	1	1
High Default		0.756	1	0.43	1	1
Low Default		0.943	1	0.232	1	1
Incr						
Aggregate	HD < LD	0.642	1	0.479	0	1
High Default		0.592	1	0.492	0	1
Low Default		0.651	1	0.477	0	1
Surp						
Aggregate	HD < LD	0.63	1	0.483	0	1
High Default		0.568	1	0.496	0	1
Low Default		0.64	1	0.48	0	1
EPS						
Aggregate	HD ≈ LD	0.063	0.065	0.06	0.039	0.09
High Default		0.049	0.061	0.107	0.003	0.115
Low Default		0.065	0.065	0.047	0.043	0.087
ΔEPS						
Aggregate	HD > LD	0.005	0.006	0.065	−0.011	0.02
High Default		0.012	0.012	0.121	−0.034	0.05
Low Default		0.004	0.006	0.05	−0.009	0.017
UE_EPS						
Aggregate	HD ≈ LD	0	0	0.015	−0.001	0.003
High Default		−0.002	0	0.026	−0.005	0.005
Low Default		0	0	0.013	−0.001	0.002
Rating						
Aggregate	HD > LD	7.466	7	3.297	5	9
High Default		13.632	14	1.77	12	15
Low Default		6.47	6	2.234	5	8
CFO						
Aggregate	HD < LD	0.104	0.107	0.076	0.063	0.147
High Default		0.07	0.071	0.099	0.021	0.126
Low Default		0.11	0.11	0.07	0.07	0.15
StdRoa						
Aggregate	HD > LD	0.028	0.022	0.023	0.012	0.036
High Default		0.041	0.032	0.031	0.019	0.054
Low Default		0.026	0.021	0.021	0.012	0.033
Times						
Aggregate	HD < LD	2.054	2.007	0.655	1.638	2.44
High Default		1.449	1.378	0.608	1.039	1.752
Low Default		2.152	2.081	0.609	1.76	2.495
RND						
Aggregate	HD < LD	0.018	0	0.03	0	0.026
High Default		0.005	0	0.016	0	0
Low Default		0.02	0.002	0.031	0	0.03

(continued on next page)

TABLE 4 (continued)

Variable	Significant Difference ^b	Mean	Median	Std. Dev.	Lower Quartile	Upper Quartile
<i>StdRet</i>						
Aggregate	HD > LD	0.02	0.019	0.007	0.015	0.024
High Default		0.027	0.025	0.008	0.021	0.032
Low Default		0.019	0.018	0.006	0.015	0.022
<i>BM</i>						
Aggregate	HD > LD	-0.843	-0.776	0.655	-1.214	-0.385
High Default		-0.545	-0.449	0.644	-0.888	-0.11
Low Default		-0.891	-0.819	0.645	-1.244	-0.436
<i>Size</i>						
Aggregate	HD < LD	8.374	8.358	1.367	7.444	9.33
High Default		6.912	6.746	1.168	6.045	7.679
Low Default		8.61	8.543	1.246	7.709	9.458
<i>Lev</i>						
Aggregate	HD > LD	0.248	0.233	0.129	0.154	0.326
High Default		0.373	0.381	0.153	0.271	0.482
Low Default		0.227	0.217	0.112	0.146	0.304
<i>BC</i>						
Aggregate	HD ≈ LD	1.21	1.11	0.422	0.9	1.46
High Default		1.18	1.115	0.439	0.88	1.43
Low Default		1.215	1.11	0.419	0.91	1.46
<i>Senior</i>						
Aggregate	HD < LD	0.923	1	0.266	1	1
High Default		0.512	1	0.501	0	1
Low Default		0.99	1	0.101	1	1
<i>IssueSize</i>						
Aggregate	HD < LD	5.15	5.28	0.857	4.604	5.699
High Default		4.969	5.004	0.713	4.601	5.298
Low Default		5.179	5.292	0.874	4.604	5.701
<i>Call</i>						
Aggregate	HD < LD	0.636	1	0.413	0.328	1
High Default		0.522	0.493	0.255	0.421	0.589
Low Default		0.655	1	0.43	0.164	1

^a The aggregate sample consists of 1,798 nonconvertible and fixed rate bonds issued from 1983 to 2002. The high default risk subsample consists of 250 bonds with non-investment grade ratings (BB+ or below). The low default risk subsample consists of 1,548 bonds with investment grade ratings (BBB- or above). Appendix B provides variable definitions.

^b Significant differences between the high default risk and low default risk subsamples are based on both the t-statistics of means and z-statistics of Wilcoxon Rank Sum Tests of medians ($p < 0.10$).

indicate that reporting profits and earnings increases have a stronger impact on ratings changes for high default risk firms than for low default risk firms ($p \leq 0.066$, one-tailed test). However, the effects of beating analysts' earnings forecast are similar across the two groups. Overall, the results support H2.²⁰

Panel C of Table 6 compares the relative importance of each of the three benchmarks in the aggregate and the high default risk samples (i.e., coefficients estimates in columns

²⁰ As an alternative to test H2, I constrain the coefficients of all independent variables to be the same between high and low default risk groups except the benchmark indicators. Untabulated results indicate that all three benchmarks have a stronger impact on ratings changes for the high default risk group than for the low default risk group ($p < 0.001$, one-tailed test).

TABLE 5
Significant Correlations for the Aggregate Initial Bond Spread Sample

	<i>Spread</i>	<i>Profit</i>	<i>Incr</i>	<i>Surp</i>	<i>EPS</i>	Δ <i>EPS</i>	<i>UE_EPS</i>	<i>Rating</i>	<i>CFO</i>	<i>StdRoa</i>	<i>Times</i>	<i>RND</i>	<i>StdRet</i>	<i>BM</i>	<i>Size</i>	<i>Lev</i>	<i>BC</i>	<i>Senior</i>	<i>IssueSize</i>	<i>Call</i>
<i>Spread</i>	-0.22	-0.13			-0.04			0.69	-0.23	0.21	-0.44	-0.28	0.52	0.31	-0.31	0.37	0.33	-0.39	0.04	-0.37
<i>Profit</i>	-0.26	0.32	0.10	0.48	0.35	0.09		-0.22	0.29	-0.26	0.31	0.05	-0.14	-0.15		-0.16	-0.05	0.10		
<i>Incr</i>	-0.13	0.32	0.09	0.44	0.83	0.06	0.06	-0.08	0.22	-0.18	0.19	-0.02	-0.06	-0.19	-0.03	-0.06	-0.08		-0.06	
<i>Surp</i>	-0.05	0.10	0.09	0.08	0.13	0.84	0.84	0.07	0.07		0.06		0.05	-0.09	0.04		0.06	0.04	0.08	-0.05
<i>EPS</i>	-0.14	0.67	0.41	0.11		0.55	0.16	-0.04	0.19	-0.13	0.11	0.05	-0.17	0.20	-0.11	-0.08	-0.21		-0.15	
Δ <i>EPS</i>	-0.04	0.41	0.57	0.13	0.54		0.15	0.05	0.18		0.10	-0.04		-0.08	-0.08		-0.09	-0.04	-0.08	
<i>UE_EPS</i>	-0.07	0.17	0.09	0.46	0.22	0.20		0.09				-0.05	0.06			0.04				
<i>Rating</i>	0.78	-0.25	-0.08		-0.11	0.03	0.09	-0.27	-0.28	0.26	-0.52	-0.33	0.38	0.33	-0.44	0.48		-0.43	-0.11	-0.15
<i>CFO</i>	-0.24	0.28	0.20	0.08	0.27	0.20	0.09				0.52	0.06	-0.06	-0.36		-0.17	0.04	0.13	0.04	
<i>StdRoa</i>	0.25	-0.25	-0.12		-0.15	0.04		0.26				0.08	0.22	0.07	-0.14		0.04	-0.10		-0.07
<i>Times</i>	-0.43	0.33	0.18	0.06	0.22	0.11	0.07	-0.52	0.49	-0.01		0.28	-0.15	-0.44		-0.65		0.24		
<i>RND</i>	-0.21	0.04				-0.04		-0.30	0.11	0.12	0.36		-0.19	-0.10	0.21	-0.47	-0.05	0.15	0.04	0.08
<i>StdRet</i>	0.54	-0.14	-0.07	0.04	-0.15	0.02		0.42	-0.08	0.30	-0.15	-0.06		0.04	-0.15	0.19	0.31	-0.21	0.15	-0.27
<i>BM</i>	0.26	-0.13	-0.16	-0.09	0.09	-0.06		0.30	-0.33		-0.42	-0.17	0.05		-0.14	0.10	-0.17	-0.12	-0.19	0.05
<i>Size</i>	-0.39			0.04	-0.04	-0.07	0.04	-0.51	0.04	-0.13	0.04	0.17	-0.16	-0.12		-0.08	0.15	0.34	0.60	
<i>Lev</i>	0.41	-0.17	-0.06		-0.10	-0.01	-0.04	0.51	-0.17		-0.65	-0.43	0.21	0.07	-0.14		0.05	-0.21		-0.07
<i>BC</i>	0.24	-0.03	-0.08	0.06	-0.09	-0.05	0.02	0.01	0.05	0.07	0.03	-0.03	0.34	-0.19	0.20	0.06		0.07	0.28	-0.28
<i>Senior</i>	-0.53	0.10	0.01	0.04	0.01	-0.03	0.04	-0.56	0.15	-0.09	0.24	0.12	-0.21	-0.12	0.36	-0.25	0.08		0.11	0.13
<i>IssueSize</i>	-0.03	-0.03	-0.05	0.06	-0.08	-0.07	0.03	-0.13	0.06	0.03	0.05	0.08	0.15	-0.15	0.55	-0.02	0.30	0.09		-0.24
<i>Call</i>	-0.26	-0.02	0.02	-0.06	-0.01	-0.01	0.00	-0.15	-0.04	-0.11	-0.02	0.06	-0.26	0.08	0.01	-0.07	-0.32	0.10	-0.28	

The aggregate sample consists of 1,798 nonconvertible and fixed rate bonds issued from 1983 to 2002. Spearman (Pearson) correlations are above (below) the diagonal. All correlations in the table are significant at $p < .10$ (two-tailed) significance levels. Appendix B provides variable definitions.

TABLE 6

Ordered Logit Analyses of Credit Ratings Changes on the Three Earnings Benchmarks

$$\Delta Rating_{it+1} = \alpha_0 + \alpha_1 Benchmark_{it} + \alpha_2 EarningsControl_{it} + \beta_1 \Delta CFO_{it} + \beta_2 \Delta StdRoa_{it} + \beta_3 Times_{it} + \beta_4 \Delta RND_{it} + \beta_5 \Delta StdRet_{it} + \beta_6 \Delta BM_{it} + \beta_7 \Delta Size_{it} + \beta_8 \Delta Lev_{it} + \Sigma \beta_i Year_{it} + \epsilon_{it}$$

Panel A: Results from Ordered Logit Model*

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) High	(5) Low	(6) High	(7) Low	(8) High	(9) Low	(10) Agg	(11) High
Profit	-	-0.402 (0.000)			-0.851 (0.000)	-0.175 (0.140)					-0.278 (0.001)	-0.722 (0.000)
Incr	-		-0.527 (0.000)				-0.693 (0.000)	-0.474 (0.000)			-0.452 (0.000)	-0.471 (0.000)
Surp	-			-0.208 (0.000)					-0.206 (0.019)	-0.253 (0.001)	-0.165 (0.003)	-0.098 (0.165)
EPS	-	-0.897 (0.000)			-0.653 (0.007)	-4.596 (0.000)					-0.647 (0.012)	-0.47 (0.051)
ΔEPS	-		-0.291 (0.127)				-0.056 (0.410)	-0.557 (0.234)			0.085 (0.635)	0.237 (0.839)
UE_EPS	-			-2.575 (0.000)					-3.225 (0.000)	-1.842 (0.156)	-1.322 (0.048)	-2.159 (0.006)
ΔCFO	-	-0.386 (0.050)	-0.134 (0.291)	-0.626 (0.004)	-0.323 (0.137)	0.109 (0.601)	-0.201 (0.258)	0.139 (0.639)	-0.701 (0.009)	-0.335 (0.192)	-0.132 (0.292)	-0.197 (0.260)
ΔStdRoa	+	1.813 (0.041)	2.602 (0.006)	3.164 (0.001)	-0.2 (0.570)	2.6 (0.112)	1.045 (0.173)	4.448 (0.015)	1.11 (0.155)	5.773 (0.003)	1.638 (0.056)	-0.245 (0.586)
ΔTimes	-	-0.466 (0.000)	-0.42 (0.000)	-0.537 (0.000)	-0.316 (0.000)	-0.582 (0.000)	-0.299 (0.000)	-0.584 (0.000)	-0.418 (0.000)	-0.717 (0.000)	-0.38 (0.000)	-0.242 (0.001)
ΔRND	+	2.136 (0.047)	1.712 (0.088)	2.386 (0.027)	1.7 (0.142)	1.332 (0.232)	1.345 (0.186)	1.009 (0.291)	2.028 (0.086)	1.843 (0.163)	1.534 (0.115)	1.095 (0.247)
ΔStdRet	+	15.06 (0.000)	19.543 (0.000)	19.84 (0.000)	12.603 (0.006)	21.178 (0.002)	19.962 (0.000)	23.268 (0.001)	19.945 (0.000)	24.197 (0.000)	14.462 (0.000)	12.026 (0.008)
ΔBM	+	0.765 (0.000)	0.68 (0.000)	0.742 (0.000)	0.563 (0.000)	1.118 (0.000)	0.439 (0.000)	1.069 (0.000)	0.511 (0.000)	1.133 (0.000)	0.706 (0.000)	0.521 (0.000)

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TABLE 6 (continued)

$\Delta Size$	-	-1.405 (0.000)	-1.509 (0.000)	-1.645 (0.000)	-0.86 (0.000)	-1.676 (0.000)	-1.202 (0.000)	-1.811 (0.000)	-1.329 (0.000)	-1.986 (0.000)	-1.288 (0.000)	-0.766 (0.000)
ΔLev	+	3.698 (0.000)	3.765 (0.000)	3.944 (0.000)	3.245 (0.000)	3.825 (0.000)	3.499 (0.000)	3.864 (0.000)	3.683 (0.000)	4.071 (0.000)	3.588 (0.000)	3.161 (0.000)
Observations		8878	8878	8878	2848	6030	2848	6030	2848	6030	8878	2848
Generalized R ²		0.115	0.116	0.111	0.172	0.113	0.154	0.109	0.149	0.103	0.124	0.182

Panel B: Testing the Effect of Beating Benchmarks between High and Low Default Risk (H2)

	Profit Benchmark	Earnings Increase Benchmark	Analysts' Forecast Benchmark
Coefficient Difference between High and Low Default Risk Group ^b (p-value)	-0.676 (0.001)	-0.175 (0.066)	0.047 (0.644)

Panel C: Testing the Relative Importance of Beating Each Benchmarks (H3)

	H0: Profit $\geq$ Incr Difference ^c (p-value)	H0: Profit $\geq$ Surp Difference ^c (p-value)	H0: Incr = Surp Difference ^c (p-value)
Aggregate Sample	0.174 (0.933)	-0.113 (0.158)	-0.287 (0.002)
High Default Sample	-0.251 (0.085)	-0.623 (0.000)	-0.373 (0.026)

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TABLE 6 (continued)

Panel D: Changes in Predicted Probabilities of Ratings Upgrade and Downgrades When the Benchmark Indicators Change from 0 to 1, Holding All Other Variables at Mean for the High Default Risk Sample

	Average Effects of Beating an Earnings Benchmark ^d		Incremental Effects of Beating an Earnings Benchmark after Controlling for the Other Two Benchmarks ^e	
	Ratings Upgrade	Ratings Downgrade	Ratings Upgrade	Ratings Downgrade
<i>Profit</i> = 0	6%	18%	6%	17%
<i>Profit</i> = 1	13%	9%	12%	9%
Effects of Reporting Profits	7%	-9%	6%	-8%
<i>Incr</i> = 0	7%	16%	8%	14%
<i>Incr</i> = 1	13%	9%	12%	9%
Effects of Reporting Earnings Increases	6%	-7%	4%	-5%
<i>Surp</i> = 0	9%	13%	9%	12%
<i>Surp</i> = 1	11%	11%	10%	11%
Effects of Beat Analysts' Earnings Forecasts	2%	-2%	1%	-1%

^a Robust p-values are in parentheses. I calculate p-values using one-tailed tests. The estimated coefficients for the constant term and year dummy variables are not reported. Appendix B provides variable definitions.

^b The coefficient difference is the benchmark coefficient from high default risk group minus that from the low default risk group from Panel A. I calculate p-values using a one-tailed Chi-Square test.

^c Difference in the earnings benchmark coefficients from columns (10) or (11) of Panel A. I calculate p-values using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

^d The average effect of beating an earnings benchmark is based on the rating change model when one benchmark indicator is included (i.e., models in columns 4, 6, and 8 of Panel A).

^e The incremental effect of beating an earnings benchmark is based on the rating change model when all three benchmark indicators are included (i.e., model in column 11 of Panel A).

(10) and (11) of Panel A). In the aggregate sample, the effect of reporting profits is similar to the effects of the other two benchmarks. However, in the high default risk sample, reporting a profit has a stronger effect than reporting earnings increases or beating analysts' earnings forecast ($p = 0.085$ and $p < 0.001$, respectively; one-tailed test). Panel C also shows that reporting an earnings increase has a significantly stronger effect than beating analysts' earnings forecasts ($p \leq 0.013$, two-tailed test). Overall, the results suggest that the relative importance of beating the various earnings benchmarks in the debt market differs from the equity market. In contrast to the equity market, both reporting profits and reporting earnings increases have a stronger impact on firms' credit ratings than beating analysts' earnings forecasts.

To assess the economic significance of beating earnings benchmarks on ratings changes, I calculate the change in predicted probabilities of ratings upgrades and downgrades when the benchmark indicators switch from 0 to 1, holding all other variables at their means for the high default risk sample. Panel D indicates that reporting a profit increases a firm's probability of rating upgrades from 6 percent to 13 percent (i.e., a 116 percent increase) and decreases its probability of rating downgrades from 18 percent to 9 percent (i.e., a 50 percent decrease). Reporting an earnings increase raises a firm's probability of rating upgrades from 7 percent to 13 percent (i.e., an 85 percent increase) and decreases its probability of rating downgrade from 16 percent to 9 percent (i.e., a 44 percent decrease). In contrast, beating analysts' earnings forecast only increases a firm's probability of rating upgrades from 9 percent to 11 percent (i.e., a 22 percent increase) and decreases its probability of rating downgrades from 13 percent to 11 percent (i.e., a 15 percent decrease). Finally, reporting a profit still increases (decreases) a firm's probability of rating upgrades (downgrades) by 6 percent (8 percent) after holding the other two benchmarks constant.²¹

Table 6, Panel A indicates that the coefficient estimates on most control variables are significant in the expected directions. In particular, the coefficients for $\Delta StdRoa_{it}$, ΔRND_{it} , $\Delta StdRet_{it}$, ΔBM_{it} , and ΔLev_{it} are positive and significant. Likewise, the coefficients for ΔCFO_{it} , $\Delta Times_{it}$, and $\Delta Size_{it}$ are negative and significant. In general, the coefficients for the continuous earnings variables (EPS_{it} , ΔEPS_{it} , and UE_EPS_{it}) are negative and significant.

Sensitivity Analyses—Credit-Rating Analyses

Kasznik and McNichols (2002) and Xue (2005) suggest that firms that beat the three earnings benchmarks have superior future earnings performance, which might explain the lower cost of debt documented in Table 6. To account for this possibility, I re-estimate model (1) after controlling for average *ROA* from year $t+1$ to $t+2$, average *ROA* from year $t+1$ to $t+3$, average *ChgROA* from year t to $t+2$, or average *ChgROA* from year t to $t+3$ one at a time. Results (not tabulated) generally are consistent with those reported in Table 6, suggesting that superior future earnings performance does not explain the lower cost of debt associated with beating earnings benchmarks.

The rating change model is effective in mitigating omitted correlated variables and serial correlations among error terms. As an alternative, I estimate a credit-rating level

²¹ As an alternative to assess the relative importance of the six earnings variables (i.e., three earnings benchmarks and three continuous earnings variables) in explaining ratings changes, I start with a base model that includes all six earnings variables and delete one earnings variable at a time for the high default risk sample (i.e., the model in column 11 of Panel A). The results continue to show that analysts' earnings forecast benchmark is the least important benchmark, while the profit benchmark is the most important one. Specifically, the generalized R^2 only declines by 0.19 percent when I delete analysts' earnings forecast benchmark, 3.2 percent when I delete the earnings increase benchmark, and 6.5 percent when I delete the profit benchmark from the base model.

model with lagged credit ratings as an independent variable to control for omitted correlated variables and autocorrelation of the error terms. According to Wooldridge (2000, 300), "Using a lagged dependent variable in a cross-sectional equation provides a simple way to account for historical factors that cause *current* differences in the dependent variable that are difficult to account for in other ways" (emphasis in the original). Both the dependent variable, $Rating_{it+1}$, and the independent variables represent the "level" specifications of the variables in model (1) (e.g., this analysis includes CFO instead of ΔCFO).

Table 7 reports the estimation results for the high default risk sample (i.e., firms with non-investment grade ratings). Columns (1)–(4) indicate that beating any of the three earnings benchmark is associated with a better one-year-ahead credit rating ($p \leq 0.036$, one-tailed test). Untabulated tests based on column (4) also shows that reporting profit has the largest impact on credit rating than the other two benchmarks. For comparison, I report results from a rating level model without lagged credit ratings in columns (5)–(7). The results indicate that only reporting a profit is associated with a better future rating. The coefficients of reporting earnings increases and beating analysts' earnings forecast are negative but not significant at conventional level ($p = 0.20$, one-tailed test).

As another robustness test, I estimate model (1) using Moody's ratings changes as the dependent variable. Recent evidence on credit ratings *differences* (i.e., split ratings) indicates that Moody's ratings may be systematically different from S&P's (Morgan 2002). In my sample, the association of annual rating *changes* between Moody's and S&P's is only about 48 percent. Untabulated results show that beating each of the three earnings benchmarks is associated with a higher likelihood of a Moody's ratings upgrade, and the profit benchmark remains the most important benchmark.

The Initial Bond Yield Spread Sample

Table 8, Panel A presents the results of estimating model (2) for the bond issues sample.²² Columns (1) and (3) indicate that consistent with H1, beating the profit benchmark and the most recent analyst's earnings forecast is associated with a smaller yield spread in the aggregate sample. In particular, firms reporting profits have a bond yield spread that is 28 basis points less than those reporting losses ($p = 0.001$, one-tailed test).²³ Firms beating the analyst's earnings forecast have a bond yield spread that is seven basis points less than firms missing analysts' earnings forecasts ($p = 0.007$, one-tailed test). Even after controlling for the other benchmarks, column (10) shows that both reporting profits and beating analysts' earnings forecasts are incrementally associated with a smaller initial bond yield spread ($p \leq 0.009$, one-tailed test). In contrast, column (6) indicates that reporting earnings increases is associated with a lower yield spread only in the high default risk sample ($p = 0.056$, one-tailed test).²⁴

²² To improve estimation efficiency, Shi (2003) jointly estimates a new bond yield and a new bond-rating model through Seemingly Unrelated Regressions (SUR). If the errors terms from the new bond yield model and the new bond-rating model are correlated, SUR estimation improves efficiency relative to OLS regressions (i.e., smaller standard errors and higher significant levels for coefficient estimates). Following Shi (2003), I jointly estimate model (2) and a new bond-rating model with the $Rating_{it}$ as the dependent variable through the SUR estimation. Inferences for model (2) are unchanged.

²³ The median new bond issue in my sample is \$196 million with a ten-year maturity. Evidence that a firm reporting profits, on average, has a bond yield spread that is 28 points lower suggests that firms that beat the profit benchmark pay \$5,488,000 less in interest over the median bond term.

²⁴ Inferences remain the same when I re-estimate model (2) after controlling for average ROA from year $t+1$ to $t+2$, average ROA from year $t+1$ to $t+3$, average $ChgROA$ from year t to $t+2$, or average $ChgROA$ from year t to $t+3$ one at a time. Thus, superior future earnings performance does not explain the association between the yield spread and beating earnings benchmarks.

TABLE 7
Ordered Logit Analyses of Credit Ratings Levels on the Three Earnings Benchmarks

$$Rating_{it+1} = \alpha_0 + \alpha_1 Benchmark_u + \alpha_2 EarningsControl_u + \alpha_3 Rating_u + \beta_1 CFO_u + \beta_2 StdRoa_u + \beta_3 Times_u + \beta_4 RND_u + \beta_5 StdRet_u + \beta_6 BM_u + \beta_7 Size_u + \beta_8 Lev_u + \Sigma \beta_i Year_u + \epsilon_u$$

Variable	Predicted Sign	With LagRating			Without LagRating				
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Profit	-	-0.636 (0.000)			-0.522 (0.000)	-0.572 (0.000)			-0.587 (0.000)
Incr	-		-0.433 (0.000)		-0.3 (0.000)		-0.064 (0.202)		0.098 (0.896)
Surp	-			-0.143 (0.036)	-0.063 (0.219)			-0.066 (0.202)	-0.021 (0.395)
EPS	-	-0.808 (0.003)			-0.57 (0.027)	-0.853 (0.001)			-1.172 (0.000)
ΔEPS	-		-0.34 (0.039)		0.009 (0.519)		0.03 (0.606)		0.574 (1.000)
UE_EPS	-			-2.957 (0.000)	-2.091 (0.010)			-3.664 (0.000)	-2.757 (0.001)
Rating	+	2.658 (0.000)	2.683 (0.000)	2.647 (0.000)	2.679 (0.000)				
CFO	-	-0.634 (0.037)	-0.763 (0.018)	-1.275 (0.000)	-0.552 (0.065)	0.132 (0.618)	-0.495 (0.127)	-0.5 (0.120)	-0.1 (0.411)

(continued on next page)

TABLE 7 (continued)

Variable	Predicted Sign	With Lag Rating				Without Lag Rating			
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>StdRoa</i>	+	-0.433 (0.790)	-0.146 (0.608)	-0.114 (0.580)	-0.404 (0.777)	0.916 (0.168)	1.211 (0.103)	1.279 (0.088)	0.795 (0.217)
<i>Times</i>	-	-0.24 (0.000)	-0.415 (0.000)	-0.426 (0.000)	-0.242 (0.000)	-0.69 (0.000)	-0.9 (0.000)	-0.88 (0.000)	-0.659 (0.000)
<i>RND</i>	+	2.276 (0.001)	2.002 (0.006)	2.459 (0.001)	2.278 (0.001)	4.862 (0.000)	4.924 (0.000)	5.188 (0.000)	5.323 (0.000)
<i>StdRet</i>	+	17.221 (0.000)	23.68 (0.000)	23.238 (0.000)	17.306 (0.000)	57.318 (0.000)	64.379 (0.000)	62.919 (0.000)	54.152 (0.000)
<i>BM</i>	+	0.503 (0.000)	0.465 (0.000)	0.493 (0.000)	0.475 (0.000)	0.301 (0.000)	0.302 (0.000)	0.297 (0.000)	0.318 (0.000)
<i>Size</i>	-	-0.232 (0.000)	-0.196 (0.000)	-0.182 (0.000)	-0.222 (0.000)	-0.724 (0.000)	-0.686 (0.000)	-0.679 (0.000)	-0.719 (0.000)
<i>Lev</i>	+	1.183 (0.000)	0.881 (0.001)	1.003 (0.000)	1.088 (0.000)	1.48 (0.000)	1.335 (0.001)	1.343 (0.001)	1.574 (0.000)
Observations		2848	2848	2848	2848	2848	2848	2848	2848
Generalized R ²		0.844	0.843	0.841	0.839	0.471	0.454	0.461	0.478

The sample consists of 2,848 firm year observations whose S&P senior debt ratings at year *t* are non-investment grade (BB+ or below). The dependent variable, *Rating_{t+1}*, is firm *t*'s senior debt rating in year *t*+1. All independent variables are defined in Appendix B. Robust p-values from one-tailed test are in parentheses. The estimated coefficients for the constant term and year dummy variables are not reported.

TABLE 8
Regression of the Initial Bond Yield Spread on the Three Earnings Benchmarks

$$\text{Spread}_{i,t+1} = \alpha_0 + \alpha_1 \text{Benchmark}_{i,t} + \alpha_2 \text{EarningsControl}_{i,t} + \beta_1 \text{Rating}_{i,t} + \beta_2 \text{CFO}_{i,t} + \beta_3 \text{StdRoa}_{i,t} + \beta_4 \text{Times}_{i,t} + \beta_5 \text{RND}_{i,t} + \beta_6 \text{StdRet}_{i,t} + \beta_7 \text{BM}_{i,t} + \beta_8 \text{Size}_{i,t} + \beta_9 \text{Lev}_{i,t} + \beta_{10} \text{BC}_{i,t} + \beta_{11} \text{Senior}_{i,t} + \beta_{12} \text{IssueSize}_{i,t} + \beta_{13} \text{Call}_{i,t} + \varepsilon_{i,t}$$

Panel A: Results from OLS^a

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) High	(5) Low	(6) High	(7) Low	(8) High	(9) Low	(10) Agg	(11) High
Profit	-	-0.275 (0.001)			-0.178 (0.199)	-0.115 (0.049)					-0.255 (0.002)	-0.11 (0.307)
Incr	-		0.006 (0.567)				-0.239 (0.056)	0.01 (0.632)			-0.002 (0.481)	-0.178 (0.133)
Surp	-			-0.082 (0.007)					-0.224 (0.044)	-0.021 (0.230)	-0.08 (0.009)	-0.2 (0.066)
EPS	-	0.082 (0.582)			-0.809 (0.170)	0.808 (0.993)					0.606 (0.921)	0.261 (0.615)
ΔEPS	-		-0.89 (0.011)				-0.525 (0.199)	-0.517 (0.038)			-0.537 (0.100)	-0.243 (0.349)
UE_EPS	-			-3.097 (0.010)					-4.303 (0.037)	-1.022 (0.176)	-2.52 (0.033)	-3.204 (0.110)
Rating	+	0.167 (0.000)	0.171 (0.000)	0.172 (0.000)	0.38 (0.000)	0.077 (0.000)	0.383 (0.000)	0.079 (0.000)	0.397 (0.000)	0.079 (0.000)	0.171 (0.000)	0.389 (0.000)
CFO	-	0.068 (0.604)	0.075 (0.614)	0.001 (0.502)	-0.44 (0.237)	0.086 (0.684)	-0.575 (0.168)	0.219 (0.882)	-0.387 (0.263)	0.16 (0.816)	0.112 (0.670)	-0.294 (0.312)
StdRoa	+	0.142 (0.423)	0.904 (0.122)	0.742 (0.158)	1.822 (0.168)	0.009 (0.494)	2.787 (0.071)	0.143 (0.403)	2.839 (0.053)	0.103 (0.428)	0.303 (0.346)	2.416 (0.119)

(continued on next page)

TABLE 8 (continued)

Variable	Predicted Sign	(1) <i>Profit</i>	(2) <i>Incr</i>	(3) <i>Surp</i>	(4) High Low	(5) High Low	(6) High Low	(7) High Low	(8) High Low	(9) Low	(10) Agg	(11) High
<i>Times</i>	-	-0.123 (0.002)	-0.145 (0.001)	-0.143 (0.000)	0.134 (0.827)	-0.123 (0.000)	0.082 (0.717)	-0.117 (0.000)	0.072 (0.695)	-0.118 (0.000)	-0.125 (0.002)	0.098 (0.755)
<i>RND</i>	+	1.303 (0.011)	1.281 (0.013)	1.35 (0.009)	4.04 (0.113)	0.277 (0.230)	2.64 (0.219)	0.259 (0.247)	3.852 (0.144)	0.291 (0.220)	1.19 (0.018)	2.976 (0.197)
<i>StdRet</i>	+	0.29 (0.000)	0.293 (0.000)	0.294 (0.000)	0.228 (0.003)	0.192 (0.000)	0.216 (0.004)	0.186 (0.000)	0.241 (0.001)	0.186 (0.000)	0.299 (0.000)	0.238 (0.002)
<i>BM</i>	+	0.184 (0.000)	0.18 (0.000)	0.179 (0.000)	0.411 (0.000)	0.195 (0.000)	0.371 (0.000)	0.212 (0.000)	0.415 (0.000)	0.21 (0.000)	0.167 (0.000)	0.39 (0.000)
<i>Size</i>	-	-0.096 (0.000)	-0.094 (0.000)	-0.091 (0.000)	-0.095 (0.085)	-0.069 (0.000)	-0.1 (0.067)	-0.066 (0.000)	-0.072 (0.131)	-0.066 (0.000)	-0.091 (0.000)	-0.091 (0.085)
<i>Lev</i>	+	0.093 (0.321)	0.07 (0.366)	0.071 (0.359)	0.402 (0.207)	0.077 (0.299)	0.223 (0.325)	0.084 (0.284)	0.285 (0.271)	0.089 (0.272)	0.066 (0.368)	0.259 (0.292)
<i>BC</i>	+	0.575 (0.000)	0.568 (0.000)	0.578 (0.000)	0.936 (0.000)	0.62 (0.000)	0.895 (0.000)	0.61 (0.000)	0.91 (0.000)	0.613 (0.000)	0.571 (0.000)	0.894 (0.000)
<i>Senior</i>	-	-0.758 (0.000)	-0.752 (0.000)	-0.735 (0.000)	0.064 (0.689)	-0.233 (0.074)	0.099 (0.782)	-0.26 (0.054)	0.114 (0.823)	-0.25 (0.062)	-0.736 (0.000)	0.109 (0.814)
<i>IssueSize</i>	?	0.064 (0.003)	0.062 (0.004)	0.067 (0.002)	0.046 (0.647)	0.045 (0.011)	0.040 (0.687)	0.041 (0.022)	0.066 (0.498)	0.043 (0.015)	0.062 (0.004)	0.054 (0.580)
<i>Call</i>	-	-0.179 (0.000)	-0.172 (0.000)	-0.171 (0.000)	0.396 (0.977)	-0.234 (0.000)	0.436 (0.983)	-0.24 (0.000)	0.41 (0.979)	-0.239 (0.000)	-0.177 (0.000)	0.44 (0.985)
Observations		1798	1798	1798	250	1548	250	1548	250	1548	1798	250
Adjusted R ²		0.725	0.724	0.726	0.498	0.530	0.505	0.530	0.514	0.529	0.728	0.515

(continued on next page)

TABLE 8 (continued)

Panel B: Testing the Effect of Beating Benchmarks between High and Low Default Risk

	Profit Benchmark	Earnings Increase Benchmark	Analysts' Forecast Benchmark
Coefficient Difference between High and Low Default Risk Groups ^b (p-value)	-0.063 (0.381)	-0.249 (0.054)	-0.204 (0.061)

Panel C: Testing the Relative Importance of Earnings Benchmarks in the Bond Issues Sample

	H0: <i>Profit</i> ≥ <i>Incr</i> Difference ^c (p-value)	H0: <i>Profit</i> ≥ <i>Surp</i> Difference ^c (p-value)	H0: <i>Incr</i> = <i>Surp</i> Difference ^c (p-value)
Aggregate Sample	-0.253 (0.003)	-0.175 (0.033)	0.078 (0.125)
High Default Sample	0.067 (0.590)	0.090 (0.631)	0.022 (0.913)

^a Robust p-values from one-tailed test (except for *IssueSize*) are in parentheses. Estimates for constant term are not reported. I divide the estimated coefficient for *SizeRet* by 100 to facilitate interpretation. Appendix B provides variable definitions.

^b The coefficient difference is the benchmark coefficient from high default risk minus that from the low default risk group from Panel A. I calculate p-values using a one-tailed t-test.

^c Difference in the earnings benchmark coefficients from columns (10) or (11) of Panel A. I calculate p-values using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Consistent with H2, columns (6)–(9) of Panel A of Table 8 show that reporting earnings increases and beating analysts' earnings forecasts are associated with a lower yield spread *only* for the high default risk sample ($p \leq 0.056$, one-tailed test). In addition, Panel B indicates that the difference in the costs of debt effects between high and low default risk sample for these two benchmarks are statistically significant ($p \leq 0.061$, one-tailed test). Inconsistent with H2, reporting profits does not have a more pronounced effect for the high default risk sample. Further analysis indicates the insignificant coefficients of both *Profit* and *EPS* in column (4) are likely caused by the lack of power due to small sample size (i.e., estimating 16 variables using 250 observations) and the collinearity between *Profit* and *EPS* (Pearson correlation between the two is 0.80). Eliminating either *Profit* or *EPS* from the model does not affect the R^2 but results in the remaining variable being significant in the expected direction. When I exclude *EPS* from the analysis of high default risk sample, the effect of reporting profits becomes significantly associated with a lower cost of debt and is more pronounced than that for the low default risk sample.²⁵

Panel C of Table 8 compares the relative importance of beating each benchmark in the aggregate sample and the high default risk sample. Consistent with H3, reporting profits has the largest effect on the initial bond yield spread for the aggregate sample ($p \leq 0.033$, one-tailed test).²⁶ For the high default risk sample, there is no significant difference among the effects of reporting profits, earnings increases, or beating analysts earnings forecast.

Panel A of Table 8 also indicates that the majority of the control variables are significant with expected signs. In particular, the coefficients for *Rating_{it}*, R&D expenses (*RND_{it}*), variance of stock returns (*StdRet_{it}*), book-to-market ratios (*BM_{it}*), and the average yield differences between Moody's AAA and Treasury bond (*BC_{it}*) are positive and significant. Likewise, the coefficients for *Times_{it}*, *Size_{it}*, *Senior_{it}*, and *Call_{it}* are negative and significant. The coefficient for *IssueSize_{it}* is positive and significant in most cases, indicating that in my sample the size of the bond issue reflects the overall debt burden more than the bond's liquidity. The coefficients for ΔEPS_{it} and UE_EPS_{it} are in general negative and significant, indicating reporting more earnings increases and more positive earnings surprises are associated with lower yield spread.

Not all firms issue public debt. To assess whether a potential self-selection bias affects my inferences in the new bond sample, I employ the Heckman two-stage procedure. For the first stage probit selection model, the dependent variable equals 1 if the observation enters the new bond analysis, and 0 otherwise. The independent variables include most firm-year specific variables in model (2) (i.e., *Rating*, *CFO*, *StdRoa*, *Times*, *RND*, *StdRet*, *BM*, *Size*, and *Leverage*) plus two exogenous indicator variables: whether a firm's stock is included in the S&P 500 index and whether a firm is traded on the NYSE. Faulkender and Petersen (2006) use these last two variables to measure a firm's visibility and argue that

²⁵ Eliminating some variables is one approach suggested by Maddala (2001) and Kennedy (1998) to deal with multicollinearity. Another approach is to enlarge the sample size. Indeed, if I classify more observations into the high default risk sample (such as classifying bonds into approximately equal-sized high and low default risk groups), the coefficient of *Profit* becomes significant when *EPS* is in the model and is more pronounced than that for the low default risk sample.

²⁶ To assess the relative contributions of the six earnings variables (i.e., three earnings benchmarks and three continuous earnings variables) in explaining credit spread, I delete one earnings variable at a time from a benchmark model that includes all six earnings variables for the aggregate sample (i.e., the model in column 10 of Panel A). Untabulated results indicate that the coefficient estimates of a benchmark indicator change the most when its corresponding continuous earnings variables are deleted, and vice versa. The results also indicate that consistent with H3 the adjusted R^2 declines the most when *Profit* is deleted.

the more well known or visible a firm is, the more likely a firm issues public debt.²⁷ From the first stage probit model I calculate the inverse-Mills ratio and include it as an additional variable in the second stage model (i.e., model (2)). Inferences regarding the three benchmarks remain the same, suggesting that the potential self-selection bias does not drive the effects of beating earnings benchmarks on initial bond yield spread.

Earnings Management and Beating Earnings Benchmarks

The above analyses capture the *average* effects of meeting/beating benchmarks regardless of the method used to beat the benchmarks. For firms that likely beat earnings benchmarks through earnings management, the debt market benefits may be attenuated. To test this possibility, I construct a dichotomous indicator variable *EM* (Earnings Management) and interact *EM* with each of the three benchmark indicators in model (1) of the aggregate credit ratings analysis.²⁸ The interaction terms capture the *incremental* effect on the cost of debt of beating benchmarks through earnings manipulation. Due to the lack of consensus regarding the most appropriate *EM* proxy, I use four alternative proxies for earnings management: abnormal accruals from a forward-looking model (Dechow et al. 2003), abnormal accruals from the modified Jones model, abnormal cash flow from operations (Roychowdhury 2006) and the unexpected change of effective tax rate (Dhaliwal et al. 2004). Prior research finds firms could meet or beat earnings targets through higher abnormal accruals, lower abnormal cash expenditure, and lower tax expenses in the fourth quarter. Accordingly, I code *EM* equal to 1 if the abnormal accruals (abnormal CFO and unexpected *ETR*) is in the top (bottom) 20 percent of the industry/year ranks among the sample firms.²⁹

I find that in 11 out of 12 specifications (four *EM* measures times three benchmarks) the interaction terms (*EM * Benchmark*) are significantly positive, which is consistent with rating agencies discounting the effects of beating benchmarks if firms meet benchmarks through earnings management. However, the results also indicate that in most cases these firms still enjoy a reduction in the cost of debt (in nine out of 12 specifications, the sum of a benchmark indicator and the *EM* interaction term is still significantly negative). Overall, these tests indicate that beating earnings benchmarks is associated with a lower cost of

²⁷ The first stage selection model includes 10,740 observations. The Hosmer-Lemeshow test of goodness-of-fit does not reject the null hypothesis that the model fits the current data (p-value = 0.34). In addition, the area under the ROC curve is 0.74, which indicates that the selection model is relatively accurate in classifying observations into firm-years issuing public bonds and firm-years not issuing public bonds (the area under an ROC curve ranges from 0.5, no discrimination of the outcomes, to 1, a perfect discrimination). Overall, the measures of the model fit and classification accuracy indicate that the first-stage selection model is reasonably successful.

²⁸ Anthony et al. (2006) show that there is little evidence that firms with investment grade ratings manipulate earnings prior to issuing nonconvertible bonds. Because my initial bond yield sample only includes nonconvertible bond issues and their ratings are overwhelmingly (i.e., 86 percent) investment grade, this sample is not a powerful setting to test whether earnings management attenuates the effects of beating earnings benchmarks on the cost of debt. Consistent with this expectation, when I use the aggregate initial bond yields sample (from 1254 to 1587 observations depending on the *EM* proxy) to test whether earnings management attenuates the effects of beating earnings benchmarks on the cost of debt, seven of 12 interactions are positive, but only one of them is statistically significant. When I limit the analyses to the less than 200 non-investment grade bonds in the sample (Anthony et al. [2006] find some evidence of earnings management in this sample), 11 of 12 interactions are positive, and two of them are statistically significant.

²⁹ As an alternative to identify earnings management firms, I adjust the reported earnings by estimated abnormal accruals (abnormal CFO and unexpected tax expenses). I then code *EM* = 1 if a firm that beats or meets a benchmark using reported earnings but misses a benchmark using adjusted earnings. Inferences remain the same.

debt, but that advantage is attenuated if the firm beats the benchmark by manipulating earnings.

Changes in the Yield Spread for Existing Bonds

As an additional robust test, I examine whether beating earnings benchmarks affects an *existing* bond's yield spreads around earnings announcements. As argued by Yu (2005), existing bonds are likely to be less sensitive to firm information than new bonds, where there is more information asymmetry. Yet a "change" analysis helps to buttress the inference that beating earnings benchmarks *causes* the change in bond spreads. I calculate the change in a bond's yield spread around a firm's earnings announcement date as the difference in the average bond spread of all transactions between 30 days before and 30 days after the earnings announcement date.³⁰ For this analysis, I represent each independent variable in model (2) in a changes specification except for the earnings benchmarks and the three corresponding continuous earnings variables.

Results in Table 9 indicate that reporting profits is associated with a 15 basis points decrease in yield spread around a firm's earnings announcement date.³¹ No other benchmark is significantly associated with a decrease in the yield spread. In sum, results indicate the secondary bond market provides an additional incentive for firms to report profits.

VI. CONCLUSIONS

This study investigates whether beating earnings benchmarks reduces a firm's cost of debt. I measure a firm's cost of debt using firm credit ratings and initial bond yield spread. I find that firms that beat earnings benchmarks increase (decrease) the probability of a ratings upgrade (downgrade) and receive a smaller initial bond yield spread, both indicating lower cost of debt. The effect of beating earnings benchmarks generally is much stronger for firms with high default risk than for firms with low default risk. Unlike the equity market, beating the profit benchmark generally has the largest impact on a firm's cost of debt. Additional analyses indicate that the reduction of cost of debt is reduced but does not disappear for firms that likely beat earnings benchmarks through earnings management.

This study makes three contributions to the literature. First, this study provides new insights into firms' potential motivation for beating earnings benchmarks by identifying an additional economically significant benefit of beating benchmarks—reducing the cost of debt. Second, this study shows that the importance of beating the various earnings benchmarks is different in the debt market than has previously been established for equity markets. In contrast to the equity market, beating the profit benchmark usually earns the largest reduction in cost of debt. Among other implications, this evidence suggests that debt holders use a similar information set (i.e., earnings benchmarks) as equity investors but that debt holders place differing weights on accounting information than equity investors (i.e., debtholders use a different decision model).

Finally, this study provides the first systematic evidence of a discontinuous relation between credit ratings (bond spread) and earnings information around the three benchmarks.

³⁰ I collect yield spread data from Mergent's bond transaction dataset, which includes all bond transactions from 1995 to 2002 by insurance firms reported in Schedule D of the National Association of Insurance Commissioners (NAIC) regulatory filings. Insurance companies hold about one-third of the outstanding U.S. corporate bonds (Campbell and Taksler 2003), and their transactions represent at least one-quarter of all transactions in the corporate bond market (Hong and Warga 2000). Therefore, this dataset is fairly representative of the secondary bond market.

³¹ The effect of reporting profits becomes more pronounced for the high default risk sample if the full sample is separated to approximately equal-size high and low default risk samples.

TABLE 9
Regression of the Change of Yield Spreads on the Three Earnings Benchmarks

$\Delta Spread_{it+1} = \alpha_0 + \alpha_1 Benchmark_{it} + \alpha_2 EarningsControl_{it} + \beta_1 \Delta Rating_{it} + \beta_2 \Delta CFO_{it} + \beta_3 \Delta StdRoa_{it} + \beta_4 \Delta Times_{it} + \beta_5 \Delta ARND_{it}$ $+ \beta_6 \Delta StdRet_{it} + \beta_7 \Delta BM_{it} + \beta_8 \Delta Size_{it} + \beta_9 \Delta Lev_{it} + \beta_{10} \Delta BC_{it} + \varepsilon_{it}$												
Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) High	(5) Low	(6) High	(7) Low	(8) High	(9) Low	(10) Agg	(11) High
Profit	-	-0.154 (0.050)			-0.11 (0.330)	-0.12 (0.113)					-0.164 (0.039)	-0.182 (0.250)
Incr	-		0.026 (0.739)				0.334 (0.952)	0.04 (0.838)			0.049 (0.886)	0.36 (0.954)
Surp	-			-0.007 (0.434)					-0.187 (0.166)	0.022 (0.728)	-0.007 (0.434)	-0.16 (0.211)
EPS	-	-0.371 (0.188)			-0.299 (0.298)	-0.71 (0.111)					-0.154 (0.391)	0.091 (0.546)
ΔEPS	-		-0.731 (0.063)				-1.144 (0.079)	-1.153 (0.042)			-0.488 (0.208)	-1.129 (0.142)
UE_EPS	-			-0.658 (0.414)					3.027 (0.729)	-2.247 (0.260)	-0.699 (0.407)	2.44 (0.700)
ΔRating	+	0.623 (0.069)	0.636 (0.067)	0.636 (0.067)	0.86 (0.072)	0.532 (0.170)	0.928 (0.055)	0.544 (0.168)	0.904 (0.064)	0.53 (0.175)	0.625 (0.068)	0.939 (0.056)
ΔCFO	-	0.166 (0.823)	0.227 (0.879)	0.104 (0.716)	0.496 (0.720)	0.233 (0.910)	0.648 (0.748)	0.306 (0.953)	0.341 (0.651)	0.175 (0.837)	0.188 (0.835)	0.636 (0.739)

(continued on next page)

TABLE 9 (continued)

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) High	(5) Low	(6) High	(7) Low	(8) High	(9) Low	(10) Agg	(11) High
$\Delta StdRoa$	+	1.034 (0.180)	1.911 (0.065)	2.054 (0.059)	-1.065 (0.662)	2.828 (0.026)	-0.581 (0.591)	3.843 (0.012)	-0.305 (0.452)	3.939 (0.010)	1.118 (0.158)	-0.477 (0.579)
$\Delta Times$	-	0.038 (0.752)	0.043 (0.749)	-0.015 (0.386)	0.245 (0.943)	-0.063 (0.105)	0.291 (0.948)	-0.055 (0.173)	0.212 (0.932)	-0.11 (0.027)	0.055 (0.804)	0.337 (0.958)
ΔRND	+	-0.774 (0.874)	-0.544 (0.783)	-0.35 (0.691)	-4.219 (0.677)	-0.175 (0.604)	-7.76 (0.807)	0.046 (0.474)	-3.214 (0.634)	0.226 (0.375)	-0.755 (0.865)	-7.848 (0.812)
$\Delta StdRet$	+	5.037 (0.130)	5.7 (0.104)	5.831 (0.099)	7.956 (0.350)	5.155 (0.041)	8.158 (0.340)	5.715 (0.022)	11.077 (0.278)	4.821 (0.047)	5.14 (0.124)	5.467 (0.400)
ΔBM	+	0.08 (0.126)	0.084 (0.122)	0.078 (0.134)	-0.067 (0.639)	0.131 (0.018)	-0.046 (0.592)	0.142 (0.010)	-0.093 (0.702)	0.143 (0.009)	0.084 (0.119)	-0.025 (0.551)
$\Delta Size$	-	0.073 (0.786)	0.026 (0.604)	0.009 (0.537)	0.374 (0.867)	-0.008 (0.463)	0.332 (0.822)	-0.046 (0.322)	0.369 (0.836)	-0.068 (0.245)	0.073 (0.789)	0.472 (0.911)
ΔLev	+	0.112 (0.375)	0.141 (0.347)	0.09 (0.398)	2.11 (0.035)	-0.348 (0.893)	2.827 (0.019)	-0.353 (0.896)	2.304 (0.039)	-0.408 (0.922)	0.159 (0.332)	2.676 (0.017)
ΔBC	+	1.334 (0.000)	1.307 (0.000)	1.326 (0.000)	2.84 (0.001)	1.06 (0.000)	2.803 (0.001)	1.025 (0.000)	2.932 (0.001)	1.048 (0.000)	1.313 (0.000)	2.923 (0.001)
Observations		2234	2234	2234	329	1905	329	1905	329	1905	2234	329
Adjusted R ²		0.026	0.023	0.020	0.013	0.035	0.022	0.033	0.013	0.029	0.025	0.013

The full sample includes bond-year observations from Mergent's corporate bond transaction database for all nonconvertible and fixed rate bonds traded between 1995 and 2002. The high (low) default risk sample includes all investment grade (non-investment grade) bonds. The dependent variable, $\Delta Spread$, is the difference in the average bond spread of a bond's transactions between 30 days before and 30 days after the issuing firm's earnings announcement. $\Delta Rating$ is a bond's change in S&P rating before and after its issuing firm's earnings announcement. All other variables are defined in Appendix B. Robust p-values from one-tailed tests are in parentheses. The estimated coefficients for the constant term are not reported.

Results suggest that the discontinuity varies by benchmark, firm default risk, and method utilized to beat the benchmark. These findings are important for future research in modeling the determinants of credit ratings and bond spread. They also enhance our understanding of how rating agencies and debt investors use accounting information.

Readers should interpret the results of this study in light of the following limitation. Despite exhaustive controls for factors associated with a firm's cost of debt, it is possible that an unidentified correlated omitted variable systematically influences the analysis. Sensitivity analyses using alternative models and alternative cost-of-debt measures provide some assurance that indeed earnings benchmarks have a significant effect on a firm's cost of debt. On balance, therefore, the evidence supports that despite their information advantage, credit analysts and bond investors use earnings benchmarks to evaluate firm performance.

APPENDIX A

The Transformation of Ratings Letters and Sample Observations across Ratings

S&P Credit Rating Letter	<i>Rating_{it}</i> Variable	Credit Ratings Sample ^a		Bond Issues Sample ^b	
		Obs.	Percentage	Obs.	Percentage
AAA	1	222	2.50	42	2.34
AA+	2	91	1.03	13	0.72
AA	3	392	4.42	128	7.12
AA-	4	377	4.25	95	5.28
A+	5	651	7.33	215	11.96
A	6	1001	11.28	311	17.30
A-	7	731	8.23	183	10.18
BBB+	8	830	9.35	216	12.01
BBB	9	1005	11.32	216	12.01
BBB-	10	730	8.22	129	7.17
BB+	11	531	5.98	44	2.45
BB	12	644	7.25	27	1.50
BB-	13	683	7.69	45	2.50
B+	14	617	6.95	50	2.78
B	15	215	2.42	37	2.06
B-	16	94	1.06	39	2.17
CCC+ ^c	17	64	0.72	8	0.44

^a The credit ratings sample consists of 8,878 firm year observations from 1985 to 2003.

^b The bond issues sample consists of 1,798 nonconvertible and fixed rate corporate bonds issued from 1983 to 2002.

^c I combine all ratings below CCC+ with CCC+ into one category because of the limited number of observations under CCC+.

APPENDIX B

Variable Definitions

- $\Delta Rating_{it+1}$ = $Rating_{it+1} - Rating_{it}$, where $Rating_{it}$ is firm i 's Standard & Poor's senior debt rating in year t . Standard & Poor's rates a firm's debt from AAA (indicating a strong capacity to pay interest and repay principal) to D (indicating actual default). I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Thus a negative $\Delta Rating_{it+1}$ corresponds to a rating upgrade and a positive $\Delta Rating_{it+1}$ corresponds to a rating downgrade. Appendix A provides a complete conversion table between ratings letters and ratings number;
- $Spread_{it+1}$ = the yield to maturity at the issuance date for the largest bond that firm i issued in year $t+1$, minus the Treasury bond yield with similar maturity. I measure $Spread_{it+1}$ as a percentage;
- $Profit_{it}$ = 1 if firm i 's basic earnings per share before extraordinary items is greater than or equal to 0 in year t , and 0 otherwise;
- $Incr_{it}$ = 1 if firm i 's earnings per share before extraordinary items in year t is greater than or equal to that of year $t-1$, and 0 otherwise;
- $Surp_{it}$ = 1 if firm i 's earnings per share beats or meets the most recent analyst's forecast in year t , and 0 otherwise;
- EPS_{it} = firm i 's earnings per share before extraordinary items in year t divided by its stock price at the end of year $t-1$, corresponding to the profit benchmark ($Profit_{it}$);
- ΔEPS_{it} = changes in firm i 's earnings per share before extraordinary items between year t and $t-1$, divided by its stock price at the end of year $t-1$, corresponding to the earnings increase benchmark ($Incr_{it}$);
- UE_EPS_{it} = firm i 's actual earnings per share minus the single most recent analyst's forecast for year t , divided by its stock price at the end of year $t-1$, corresponding to analysts' earnings forecast benchmark ($Surp_{it}$);
- CFO_{it} = firm i 's operating cash flows at year t deflated by total assets at the beginning year, following Francis et al. (2005, 302);
- $StdRoa_{it}$ = firm i 's standard deviation of ROA calculated using five years data from year $t-4$ to t . ROA is net income before extraordinary items deflated by total assets at the beginning year;
- $Times_{it}$ = the natural log of $(1 + \text{times-to-interests-earned ratio})$, where times-to-interests-earned ratio is firm i 's operating income before depreciation and interest expense divided by interest expense both at year t ;
- RND_{it} = firm i 's research and development expense at year t deflated by deflated by total assets at the beginning year;
- $StdRet_{it}$ = the standard deviation of firm i 's daily stock returns during year t .
- BM_{it} = the natural log of firm i 's book value of equity divided by its market value of equity, both measured at the end of year t ;
- $Size_{it}$ = the natural log of firm i 's total assets at the end of year t ; and
- Lev_{it} = firm i 's long-term debt divided by total assets at the end of year t ;
- $Year_{it}$ = 1 if observation i is in year t , and 0 otherwise.

All the change variables in model (1) are defined as the first difference of the above variables, such as $\Delta CFO_{it} = CFO_{it} - CFO_{it-1}$.

- $Rating_{it}$ = in model (2), $Rating_{it}$ is bond i 's Standard & Poor's rating at the issue date. I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Appendix A provides a complete conversion table;
- BC_{it} = the difference between the average yield on Moody's Aaa bonds and the average yield of ten-year U.S. Treasury bonds for the bond issue or trade month;
- $Senior_{it}$ = 1 for senior bonds and 0 for subordinated bonds;
- $IssueSize_{it}$ = the natural log of the offering amount of the bond (in millions of dollars); and
- $Call_{it}$ = the ratio of the number of years to first call divided by the number of years to maturity. $Call_{it}$ takes the value of 1 if there is no call provision, and 0 if it is callable from the date of issuance.

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Misreporting Fundraising: How Do Nonprofit Organizations Account for Telemarketing Campaigns?

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ABSTRACT: The purpose of this study is to examine the frequency, determinants, and implications of misreported fundraising activities. We compare state telemarketing campaign reports with the associated information from nonprofits' annual Form 990 filings to directly test nonprofits' revenue and expense recognition policies. Using a conservative approach that understates the extent to which nonprofit organizations violate the reporting rules, our study indicates that 74 percent of the regulatory filings from nonprofit organizations fail to properly report telemarketing expenses. Smaller nonprofits, less monitored firms, and those with less accounting sophistication are more likely to inappropriately report telemarketing costs as a component of net revenues rather than as expenses. Nonprofits that use external accounting services are more likely to properly classify the cost of their telemarketing campaigns as professional fundraising fees.

Keywords: *efficiency; nonprofit; fundraising; telemarketing.*

Data Availability: *All data are from public sources.*

We thank Meg Nipson for her invaluable research assistance on this paper. We also thank Sinem Dogmu, Derya Honca, Michael Johnson, Tuyen Le Kha, Sai Pemmaraju, Rajal Shah, and Heun Wong for their help with this project. We thank Edward Douthett, Shirley Hunter, Chris Jones, Daniel Neely, Naomi Orensten, Kumar Visvanathan, Dan Dhaliwal (editor), two anonymous reviewers, and workshop participants at the Hauser Center on Nonprofit Organizations, Boston College, Florida Atlantic University, George Mason University, Harvard Law School, Suffolk University, The University of Alabama, University of Oregon, University of Virginia, the 2006 Annual Meeting of the Association for Research on Nonprofit Organizations and Voluntary Action (ARNOVA), the 2007 American Accounting Association (AAA) Annual Meeting, and the 2007 Midyear Meeting of the Government and Nonprofit section of AAA for their valuable comments on earlier drafts of this paper.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted December 2006
Accepted October 2007

I. INTRODUCTION

Our paper examines nonprofits that hire professional solicitors to conduct telemarketing campaigns to determine under what conditions they appropriately report the associated expenses. Using annual IRS filings (Form 990s) and matching them to the state reports of individual telemarketing campaigns, we examine three potential methods of misreporting costs. First, we investigate whether nonprofits report the proceeds from telemarketing campaigns as contribution revenue, net of the associated fundraising expense. This accounting method violates current accounting standards (both SFAS No. 117 and IRS 990 reporting rules), which require that all gross revenue and related expense amounts be separately reported. For firms that separately report revenues and expenses from telemarketing campaigns, we then examine whether the telemarketing costs are properly classified as professional fundraising fees. Finally, we explore whether the costs are fully reported as fundraising expenses rather than allocated to the program or administrative functional categories. Using a conservative approach that understates the extent to which nonprofit organizations violate the reporting rules, we find that over 74 percent of the regulatory filings fail to properly report telemarketing expenses, by either netting them against contributions or failing to characterize them as professional fundraising expenses.

Fundraising campaigns that reach a large number of potential donors can be time-consuming and complex. For this reason, nonprofit organizations often hire professional solicitation firms that have access to mailing or telephone lists and employ a larger fundraising staff than the nonprofit could afford. One concern about this method of fundraising is that it can be expensive, with the professional solicitor retaining all or most of the proceeds collected on a nonprofit's behalf (Keating et al. 2003; Strom 2003; and Bartosiewicz 2004). In an attempt to increase monitoring of this fundraising technique and improve external governance of nonprofits, several state regulators issue annual reports to make potential donors aware of these high costs. The New York State "Pennies for Charity" report, for example, reveals that professional solicitors in that state retained \$0.73 of every dollar raised through telemarketing campaigns between 1994 and 2001 (Keating et al. 2003). A second concern is that the public is being misled about the true costs of fundraising on nonprofits' annual financial filings with the IRS (Urban Institute 2004; Krishnan et al. 2006). The possibility exists that nonprofits may intentionally hide the costs associated with this often-expensive fundraising technique in order to improve financial reports and mislead donors about organizational performance.

The reliability of nonprofit financial reports is important. Prior research documents that donors and grantors use financial information to inform contribution decisions (e.g., Weisbrod and Dominguez 1986; Tinkelman 1999; Greenlee and Brown 1999; Parsons 2003) and that boards rely on this information to make executive compensation decisions (Baber et al. 2002). Further, watchdog groups issue ratings based largely on Form 990 financial information. These ratings depend heavily on efficiency ratios—such as the ratio of program expense to total expense and the ratio of fundraising expense to total contributions—to evaluate spending efficiency.¹ Our analysis indicates that by misreporting telemarketing expenses the nonprofits in our sample could understate the fundraising ratio by as much as 15 percent. Thus, misreporting telemarketing expenses has the potential to affect decisions made by users of nonprofit financial information.

An Urban Institute (2004) study also questions the quality of amounts reported as fundraising expenses on Form 990 by documenting that only 41.6 percent of nonprofits

¹ Donor reliance on financial information to guide fundraising decisions may have increased in recent years due to the creation of Guidestar, an online service that provides free access to recent Form 990 filings.

that receive contributions report fundraising expenses. Further, there is evidence that some nonprofits misreport financial information to affect how program or fundraising costs are reported. Trussel (2003) finds that certain performance indicators, such as lower surplus margins or less deferred revenue, are positively related to the likelihood that a nonprofit will manipulate its program efficiency ratio. Jones and Roberts (2006) provide evidence that nonprofits use joint cost allocations to avoid reporting changes in the program ratio. Krishnan et al. (2006) show that some nonprofits inappropriately report zero fundraising expenses despite evidence on the Internet that they engage in activities that can arguably be considered fundraising. Their paper further finds that nonprofits are more likely to report zero fundraising costs when donations or managers' pay are sensitive to changes in reported program ratios or revenue.

Our study extends this research by providing direct evidence that many nonprofits under-report fundraising costs. We examine a group of nonprofits that undeniably engage in fundraising activities (i.e., telemarketing) and study whether these organizations comply with clear rules for reporting this type of activity. Using a conservative estimate of a nonprofit's telemarketing activity, we determine that 16 percent of our larger sample of nonprofit telemarketing firm-years report no fundraising expenses. An additional 20 percent appear to under-report fundraising costs. Telemarketing costs should be properly classified as professional fundraising fees, yet the majority fails to classify these costs appropriately.

We investigate the factors associated with the three potential forms of misreporting. Prior research (Krishnan et al. 2006) suggests that misreporting may be intentional. The findings in this paper suggest that misreporting is also strongly associated with weak monitoring and accounting knowledge. Specifically, we find that smaller nonprofits, those with less accounting sophistication (those that fail to use accrual accounting and outside accounting services), and firms with less outside monitoring (those that are unaudited or have lower leverage) are more likely to inappropriately report their telemarketing costs in net revenues. Even when a firm does recognize telemarketing cost as an expense, we still find that firms are likely to misclassify the expense. First, firms with relatively more officer compensation and those that use the services of professional accountants are more likely to properly classify the costs of telemarketing campaigns as professional fundraising fees. Second, we find that smaller firms, those with relatively less officer compensation, and nonprofits with lower leverage are more likely to allocate telemarketing expenses to non-fundraising expense categories.

This research is important for a number of reasons. Prior research has either implied or inferred that financial misreporting was intentional and designed to mislead financial statement users. Misreporting, however, either could be the result of managers deliberately trying to mislead potential donors, or could be a consequence of management without the requisite accounting knowledge and reporting experience. This paper is the first to specifically consider the effect of accounting sophistication on nonprofit reporting practices.² This distinction is important because as public use and scrutiny of nonprofit financial information increases, regulators search for ways to improve the quality of nonprofit financial reports. For instance, in the wake of the Sarbanes-Oxley Act of 2002 and several nonprofit scandals, the Senate Finance Committee held hearings and is considering additional regulation of nonprofit organizations. Moreover, several states have considered or passed legislation increasing the number of nonprofits required to have annual CPA audits. Our results support the expectation that monitoring, particularly the existence of an audit, is associated with

² Accounting sophistication could be knowledge of the accounting rules applicable to completing Form 990 or a system of internal controls that mitigates the likelihood of misreporting.

higher quality reporting. The results also indicate that educating Form 990 preparers and nonprofit financial staffs can play a role in improving reporting quality.

The remainder of the paper is organized as follows. Section II describes professional fundraising campaigns and the related accounting issues. Section III presents the research hypotheses and the methodology. Section IV describes the data. The results are summarized in Section V. Section VI concludes with a summary of the study's contributions.

II. PROFESSIONAL FUNDRAISING CAMPAIGNS

The Cost of Telemarketing Campaigns

Though many nonprofits hire professional telemarketing firms to organize and complete their fundraising efforts, the use of telemarketing firms is controversial among charity officials, regulators, watchdog agencies, and donors. As a result, 39 states require nonprofit organizations that engage in charitable solicitations to register with the state. Twenty-two of these states require professional fundraisers to file reports regarding their telemarketing activities, and at least ten make telemarketer reports available to donors via the state's website.

Despite a number of arguments justifying the high costs of professional campaigns (Suhrke 2002), regulators are concerned that donors are not fully aware of or are poorly informed about the cost of telemarketing campaigns. The State of Illinois pursued one case to the U.S. Supreme Court (*Madigan v. Telemarketing Associates, Inc.*, 2003), asserting that the telemarketer led donors to believe a significant percentage of the funds would directly benefit Vietnam veterans, when, in fact, 85 percent of the funds went to the telemarketer or to cover the nonprofit's administrative costs. The Supreme Court found, in this case, that there had been "particular representations made with intent to mislead" by the telemarketing firm. However, the Court refused to define high fundraising costs as fraudulent. Similarly, the IRS attempted to revoke the tax-exempt status of the United Cancer Council using the argument that high fundraising fees charged by a professional telemarketer amounted to private inurement. However, the U.S. Court of Appeals for the Seventh Circuit rejected that claim, stating that high fundraising fees do not equate to private inurement of an inside party (*International Journal of Not-for-Profit Law [IJNL]* 1999). The courts have consistently refused to rule that high fundraising fees are excessive or illegal.

The Potential to Misreport Telemarketing Costs

Our study focuses on a less transparent issue: the representations that nonprofits make about telemarketing campaigns on their Form 990. IRS reporting rules specify that nonprofits that use professional fundraisers must report the gross proceeds from these campaigns as contributions revenue and separately report the fees retained by the professional fundraisers as "professional fundraising fees." To the extent that these expenses are incurred for fundraising rather than for programmatic or administrative costs, they are to be reported in the fundraising functional expense category.

A nonprofit can misreport the costs of these campaigns in three ways. First, nonprofits can report the net proceeds collected as contributions revenue rather than showing gross revenue as contributions and the telemarketers' fees separately as fundraising expense. Such reporting does not affect earnings, but it does affect the reported program and fundraising efficiency ratios that donors and others use for decision-making.

As an example, assume that prior to recording activities related to a telemarketing campaign, a nonprofit collects \$100,000. From these revenues, the organization spends \$60,000 on programs, \$10,000 on general administration costs, and \$20,000 on fundraising. The remaining \$10,000 is saved for future years. During the year the organization engages

a professional fundraising firm to conduct a telemarketing campaign on its behalf. The professional fundraiser collects \$25,000, retains \$18,750 (75 percent) of the proceeds as a fundraising fee, and remits the remaining \$6,250 to the nonprofit organization. The nonprofit spends an additional \$6,250 on its programs. If the nonprofit includes only the net proceeds from the telemarketing campaign as contributions revenue, it reports a program (fundraising) efficiency ratio of 69 (19) percent.³ If it follows current accounting standards and IRS reporting rules and reports the gross proceeds as revenue and the retained fee as fundraising expense, it reports less impressive program and fundraising ratios of 58 and 31 percent, respectively.⁴

Given that a nonprofit is reporting telemarketing costs as an expense rather than a deduction from revenues, it can mislead users by reporting the amounts retained by the fundraiser somewhere other than in the professional fundraising fee category. This category is an explicit line in the statement of functional expenses in Form 990, yet nonprofits sometimes report these costs on other lines, particularly "other expenses." Although this has no overall affect on the total fundraising or program ratios, it does not highlight to board members, watchdog groups, state regulators, or other users of financial information that an organization uses a professional fundraising method that may be expensive, or even controversial.

A third technique that may make it difficult to identify telemarketing costs involves allocating some telemarketing costs to program activities. Using the previous example of the \$25,000 campaign that resulted in net proceeds of \$6,250, if the entire \$18,750 telemarketing fee is reported as fundraising expense, it generates a program ratio of 58 percent. If management decides to allocate 50 percent of the telemarketing fee to program costs, then the reported program ratio increases to 66 percent, while the fundraising ratio is decreased from 31 percent to a more attractive 24 percent.⁵

III. RESEARCH QUESTIONS AND DESIGN

Categorizing Reporting Practices

Using the techniques described above, we classify a nonprofit's accounting practices in three ways: (1) as a gross or net revenue reporter, (2) as a proper expense classifier or a misclassifier, (3) as a fundraising cost nonallocator or allocator. To be classified as a gross revenue reporter, a nonprofit must report contributions on Form 990 that equal or exceed our estimate of gross telemarketing receipts and also report some fundraising expenses. This approach, however, may inappropriately classify firms with other sources of contribution revenue as gross revenue reporters, thereby biasing against finding that nonprofits misreport revenues. We define net reporters as those that report telemarketing fees as contra-revenues rather than expenses, thus excluding them from our second and third tests.

For the second test, we examine gross reporters to determine whether they properly classify their telemarketing costs as professional fundraising fees. To be categorized as a

³ Program ratio with net proceeds recorded as revenues and spent on programs: $(60,000 + 6,250)/(90,000 + 6,250) = 69$ percent. Fundraising ratio (fundraising expenses/total contributions) with no expense related to professional fundraising campaign: $20,000/(100,000 + 6,250) = 19$ percent.

⁴ Program ratio with gross proceeds recorded as revenues, retained fee recorded as fundraising expense, and net proceeds spent on programs: $(60,000 + 6,250)/(90,000 + 25,000) = 58$ percent. Fundraising ratio with retained fee recorded as fundraising expense: $(20,000 + 18,750)/(100,000 + 25,000) = 31$ percent.

⁵ Program ratio with 50 percent of retained fee allocated to program expense: $(60,000 + 6,250 + 9,375)/(90,000 + 25,000) = 66$ percent. Fundraising ratio with 50 percent of retained fee allocated to fundraising expense: $(20,000 + 9,375)/(100,000 + 25,000) = 24$ percent.

proper expense classifier, the organization must report professional fundraising fees that equal or exceed the combined fees paid to all solicitors. Finally, we categorize nonprofits by whether they allocate telemarketing fees to non-fundraising activities. We compare our estimate of telemarketing fees to total fundraising expenses on Form 990. Nonprofits that reflect telemarketing costs as fundraising expense should report total fundraising expense on Form 990 that is higher than the combined fees paid to all solicitors.

Allocation, however, is not necessarily a form of misreporting. The AICPA's SOP 87-2 and 98-2 allow the allocation of fundraising costs when fundraising activities are incurred jointly with programmatic activities (see Roberts [2005] for complete discussion of the joint cost rules). This standard is applied under limited circumstances and is typically used to decide whether and how to allocate costs related to direct-mailing fundraising activities.⁶ In fact, only 23 percent of our sample indicates that they use the joint cost standards. However, we are concerned that managers who allocate a portion of telemarketing fees out of fundraising expense may do so without proper regard for the accounting rules.

In summary, the first test examines management's decision to report gross revenue and the related telemarketing expense. The second and third tests focus on the nonprofits that properly report gross revenues and examines how these nonprofits recognize the expenses associated with telemarketing campaigns.

Hypotheses and Regression Models

State reports of the activities of professional fundraisers aim to inform donors about fundraising costs and address the concern that donors are misled by reported efficiency ratios. However, professional telemarketers report the results of individual telemarketing campaigns while nonprofits are responsible for their Form 990 filings. There is no mechanism to ensure the information reported on these two documents is consistent. Similar to Greenlee and Gordon (1998), we use actual fundraising costs reported in state telemarketing reports to investigate the veracity of fundraising costs reported by nonprofits.⁷

We hypothesize that the pressure to report favorable efficiency ratios affects most nonprofits, but that circumstances such as financial distress may affect the motivation to misreport the fundraising costs associated with telemarketing campaigns. We also expect that certain forms of nonprofit accountability and governance, such as professionally staffed organizations or external monitoring, mitigate the likelihood of inappropriate financial reporting.

To determine if the occurrence of net reporting, expense misclassification, or allocation of the costs related to professional fundraising campaigns is systematically determined by accounting sophistication, governance measures designed to protect against misreporting, or incentives to misreport, we use the following logistic regression models and adjust the standard errors and associated z-statistics for the intrafirm correlations arising from the inclusion of multiple years of observations (Huber 1967; Rogers 1993).

⁶ If a professional telemarketer delivers program information on behalf of a nonprofit organization during a fundraising campaign, then joint cost allocation rules permit allocation of some portion of the cost to program expense. Though we are not certain of the frequency of this form of program activity, we recognize that it is permissible under GAAP in some cases.

⁷ Reporting for professional telemarketing campaigns occurs annually, even if the campaigns have a duration of greater than one year.

Revenue Recognition Test

$$\begin{aligned}
 P(\text{Net Reporting}) = & \beta_0 + \beta_1 \ln \text{ASSETS} + \beta_2 \ln \text{OFFSAL} + \beta_3 \text{ACCRUAL} \\
 & + \beta_4 \text{PROFACCT} + \beta_5 \text{RESTRICTED} + \beta_6 \text{501C3} \\
 & + \beta_7 \text{AUDIT} + \beta_8 \text{LEVERAGE} + \beta_9 \text{YIELD} \\
 & + \beta_{10} \text{SECTOR} + \varepsilon.
 \end{aligned} \tag{1}$$

We first calculate $\text{REVDIFF} = \text{Contributions on Form 990} - \text{Telemarketing Gross Receipts}$. Contributions are measured using the amount reported in Part I line 1b (direct contributions) when this level of detail is available in the dataset. Otherwise, contributions are measured using Part I line 1e (total contributions). $\text{Net Reporting} = 1$ if either (a) $\text{REVDIFF} < 0$ or (b) fundraising expenses reported on the 990 = 0, and 0 otherwise.

Expense Classification Test

$$\begin{aligned}
 P(\text{Misclassifier}) = & \gamma_0 + \gamma_1 \ln \text{ASSETS} + \gamma_2 \ln \text{OFFSAL} + \gamma_3 \text{ACCRUAL} \\
 & + \gamma_4 \text{PROFACCT} + \gamma_5 \text{RESTRICTED} + \gamma_6 \text{501C3} + \gamma_7 \text{AUDIT} \\
 & + \gamma_8 \text{LEVERAGE} + \gamma_9 \text{YIELD} + \gamma_{10} \text{SECTOR} + \varepsilon.
 \end{aligned} \tag{2}$$

We calculate $\text{FEEDIFF} = \text{Professional Fundraising Fee on Form 990} - \text{Telemarketing Fees}$. The professional fundraising fee is obtained from Part II line 30 of Form 990. $\text{Misclassifier} = 1$ if $\text{FEEDIFF} < 0$, and 0 otherwise, using a sample that is composed only of firms identified as gross reporters.

Expense Allocation Test

$$\begin{aligned}
 P(\text{Allocator}) = & \lambda_0 + \lambda_1 \ln \text{ASSETS} + \lambda_2 \ln \text{OFFSAL} + \lambda_3 \text{ACCRUAL} \\
 & + \lambda_4 \text{PROFACCT} + \lambda_5 \text{RESTRICTED} + \lambda_6 \text{501C3} + \lambda_7 \text{AUDIT} \\
 & + \lambda_8 \text{LEVERAGE} + \lambda_9 \text{YIELD} + \lambda_{10} \text{SECTOR} + \varepsilon.
 \end{aligned} \tag{3}$$

We calculate $\text{FUNDEXPDIFF} = \text{Fundraising Expense on Form 990} - \text{Telemarketing Fees}$. Total fundraising expense is measured using Part I line 15 of Form 990. $\text{Allocator} = 1$ if $\text{FUNDEXPDIFF} < 0$, and 0 otherwise, using a sample that is composed only of firms identified as gross reporters.⁸

The independent variables are defined below.

Professionalism and Accounting Sophistication

According to Tinkelman (1999), larger organizations are more likely to have quality financial reports because they are more likely to have professional accounting expertise. Following Tinkelman, we use the natural logarithm of beginning total assets ($\ln \text{ASSETS}$) from Part IV line 59 column A as a proxy for organizational size. We predict negative coefficients for $\ln \text{ASSETS}$ in all three tests.⁹

⁸ Because our measure of telemarketing gross receipts is inexact, we use alternative measures of REVDIFF , FEEDIFF , and FUNDEXPDIFF to test the sensitivity of our results. These measures are discussed in the additional testing section of the paper.

⁹ An alternative proxy for size is total revenues. However, we do not use total revenues to measure size as the number is understated if an organization reports its telemarketing revenues on a net basis.

In addition to organizational size, we use the natural logarithm of officers' salaries and wages (*lnOFFSAL*) from Part II line 25 of Form 990 to measure the degree to which an organization has professional managers. Tinkelman (1999) proposes that the existence of professional (versus voluntary) management may indicate a higher level of accounting sophistication and a greater ability to produce high-quality financial reports. Baber et al. (2002), however, find that changes in executive compensation are positively associated with changes in program ratios. Managers may have an incentive to conceal fundraising costs to improve efficiency ratios and increase compensation. Thus, we do not predict the sign on the coefficients for *lnOFFSAL* for any of the tests.

We include two measures that reflect management's knowledge of generally accepted accounting principles (GAAP). Although the IRS does not require nonprofits to use GAAP when completing Form 990, knowledge and use of GAAP indicate accounting sophistication. The first measure, *ACCRUAL*, equals 1 if an organization uses accrual accounting, and 0 otherwise. We predict the coefficients on *ACCRUAL* are < 0 for all three tests.

An organization that engages an external professional accountant to assist with its reporting function is more likely to use proper accounting in its Form 990. Similar to Krishnan et al. (2006), we use an indicator variable (*PROFACCT*) that equals 1 if accounting fees on Part II line 31 of Form 990 are > 0 , and 0 otherwise. We predict negative coefficients on *PROFACCT* for all three tests.

Donors may impose restrictions on the use of their charitable contributions. These limitations can direct the use of donated funds for a specific purpose or time period, or can require that the donation is permanently invested in order to serve as an ongoing source of investment income for the nonprofit. Nonprofits that receive restricted donations are likely to be more complex in terms of their range of operations and also more sophisticated in their accounting with a wider range of services offered than nonprofits with no restricted contributions. We include a dummy variable, *RESTRICTED*, that is equal to 1 if temporarily or permanently restricted net assets from Part IV lines 68 and 69 are > 0 , and 0 otherwise. We expect the coefficients on *RESTRICTED* are negative for all three tests.

External Monitoring

Desai and Yetman (2005) and Yetman and Yetman (2006) demonstrate that nonprofit organizations are less likely to misreport when they are monitored. One form of monitoring is the federal requirement that most nonprofits organized as non-church 501(c)(3) entities must file an annual Form 990 if they earn \$25,000 or more in revenues, and make that informational filing available to any interested party upon request. Availability of financial information means these nonprofits are subjected to greater scrutiny by the donor public and/or watchdog agencies. To meet the public disclosure requirement, some nonprofits post their annual 990 filings on their own websites. Additionally, GuideStar collects the financial information from informational tax filings and has compiled a large searchable database that makes this information available, via the Internet, in a single location for a variety of stakeholders (see <http://www.guidestar.org>). In contrast, current law does not require non-501(c)(3)s and private foundations to make their Form 990s publicly available.

To date, most nonprofit accounting research examines only 501(c)(3) organizations. Our study, however, includes many non-501(c)(3) organizations, such as police and fire support organizations. We use a dummy variable, *501C3*, that equals 1 if the organization is a 501(c)(3) organization, and 0 otherwise. We predict that 501(c)(3) organizations are less likely than non-501(c)(3) organizations to misreport fundraising expenses; thus, we expect negative coefficients on *501C3* for all three tests.

Another important form of monitoring is an audit by an independent accountant. Some states require organizations that are registered in the state or doing sufficient business therein (usually based on total revenues) to submit audited financial statements. In addition to state-mandated audits, nonprofit organizations that receive a pre-defined level of federal funds must undergo an audit in compliance with the Office of Management and Budget's Circular No. A-133. We use a dummy variable, *AUDIT*, that equals 1 if either (a) an organization meets the audit requirements of the state in which it is incorporated or in which it solicits funds or (b) an organization was subjected to an A-133 audit according to the Single Audit database maintained by the Census Bureau, and 0 otherwise.¹⁰ We expect the presence of an audit (whether state or federally mandated) to be negatively associated with the occurrence of net reporting and expense misclassification. As cost allocations are permitted under GAAP, we do not expect to find a significant coefficient associated with audits in the expense allocation test.

Financial Indicators

Ideally, we would like to test whether nonprofits misreport telemarketing activities to manage the program or fundraising ratios. However, we cannot directly examine these particular financial indicators because they are affected by misreporting the revenues and expenses associated with telemarketing revenues. Because we do not have the full population of telemarketing campaigns since not all states issue reports, we cannot determine the telemarketing fees retained by all professional fundraisers for a given organization. Thus, we cannot compute program or fundraising ratios "as if" the net (gross) reporter recorded (did not record) all telemarketing expenses.

We can, however, indirectly examine management's incentive to manage efficiency ratios by investigating whether organizations with poor financial health have greater incentive to use accounting techniques to improve their financial reports. Organizations experiencing financial distress or unfavorable results may be more likely to use net reporting, or misclassify or allocate fundraising costs than those that are more financially robust or efficient. Consistent with prior research (Krishnan et al. 2006) we include leverage to measure the financial health of nonprofit organizations. We compute *LEVERAGE* as beginning total liabilities divided by beginning total assets. Lower leverage (higher solvency) indicates a more financially robust organization, which may mitigate incentives for misreporting. However, high leverage may be associated with monitoring related to debt or unearned revenue provided by creditors or donors/members, respectively, relative to firms with lower leverage. Due to these two potentially offsetting effects, we do not predict a sign on the coefficients for *LEVERAGE* for the three tests.

The primary criticism of telemarketing is that it is too expensive, thus we predict that a manager may have an incentive to hide the cost of professional fundraising activities. We measure the cost of telemarketing activities as the combined net proceeds received by the nonprofit in a year, divided by the associated gross proceeds (*YIELD*). We expect negative coefficients on *YIELD*, suggesting that a nonprofit is more likely to misreport when the yield is low.

¹⁰ We do not have a complete list of all states in which each nonprofit telemarkets. It is possible that a nonprofit's telemarketing or other activities in a particular state may require it to complete an audit, but we are unable to detect the need for the audit. Additionally, nonprofits can voluntarily choose to complete an audit. In these two cases, our *AUDIT* variable would be coded as 0 when it should be coded as 1.

Sector

We control for nonprofit sector using the first digit of the National Taxonomy of Exempt Entities (NTEE) codes. The sectors examined separately in our analysis include arts, education, health, and human services. All other types of organizations are grouped as "other."

IV. DATASETS

Telemarketing Campaign Data

To test our hypotheses, we use data from telemarketing reports available from state regulators and IRS Form 990 filings. Various states require professional fundraisers to file annual reports detailing the nonprofits for which they have generated donations, the gross proceeds collected on behalf of each organization, and the net proceeds remitted to the nonprofits. We collect these fundraising reports from the states' websites, usually from the office of the state attorney general. We retrieve state fundraising reports for California starting in 1995 and for New York in 1996. In subsequent years, we access reports from Ohio, Massachusetts, Vermont, North Carolina, Indiana, and Connecticut. Our sample includes reports from both large and small states with a variety of oversight regulations.¹¹

Most reports are only available in Adobe Acrobat (*.pdf) format, so a conversion process is required to convert the data into a machine-readable form, often requiring manual data input. After minor data cleaning, combining the state reports yields data from eight states on 20,203 telemarketing campaigns undertaken by 635 professional fundraisers on behalf of 4,248 nonprofits.

The requirements for fundraising reports vary across states. Some of the reports filed include only information about campaigns conducted within the state. Others include information about nationwide fundraising proceeds. In some cases, it appears that national figures are provided even though state-specific data is requested. Sometimes, when a campaign conducted in one year is completed in a second, the gross receipts of a single campaign are reported to the state in two consecutive years. The result of this variation of reporting practice is that a single campaign may be reported simultaneously to more than one state or that a single campaign may be reported to a state in multiple years.

To eliminate duplicate campaigns, we compare every campaign by a given telemarketing firm for a given nonprofit organization. Because there is no campaign number or other similar identifier (such as an employer identification number [EIN]), the comparison requires that we first standardize the names of the nonprofit organizations and the telemarketing firms.¹² We then compare the campaigns reported for a nonprofit-telemarketer combination. If we find multiple campaigns with equal gross receipts (within \$10), we retain a single campaign with the highest net receipts reported in the earliest year. Eliminating these duplicate campaign reports results in a reduced sample of 16,977 unique fundraising campaigns. Table 1 summarizes our dataset (campaign-level sample) of telemarketing campaigns without duplicated gross receipts during the period 1995–2004.

We recognize that the due dates for interim reports differ from state to state, thus our next concern is to address the possibility that interim and final reports are filed for the same campaigns. Several state reports do not indicate which campaign results are interim, so gross proceeds from one campaign may be included in both interim and final reports.

¹¹ For more information about the range of oversight options across different states, see Fremont-Smith (2005).

¹² For example, one campaign report may use the nonprofit name "Society for the Prevention of Cruelty to Animals," while another refers to the same organizations as "The S.P.C.A." A telemarketer may be listed as "Samano, Phillip J. & Assoc. Inc." in one report and "Phillip J. Samano and Associates, Inc." in another.

TABLE 1
Telemarketing Campaigns by Year

Year	State Reports Included	Number of Campaigns	Number of Nonprofits	Number of Telemarketers	Average Gross Proceeds	Average to Nonprofit	Median Yield
1995	California	496	416	104	285,786	116,284	24.9
1996	California, New York	1,199	944	190	263,673	96,920	25.0
1997	California, New York, Ohio	1,233	964	215	288,966	117,974	25.0
1998	California, Massachusetts, New York, Ohio, Vermont	1,884	1,284	268	244,617	90,413	25.0
1999	California, Massachusetts, North Carolina, New York, Ohio, Vermont	2,124	1,444	269	246,647	89,905	28.7***
2000	California, Massachusetts, North Carolina, New York, Ohio, Vermont	1,762	1,175	264	353,197	168,199	25.5***
2001	California, Massachusetts, North Carolina, New York, Ohio, Vermont	2,134	1,434	294	332,575	162,957	30.0***
2002	California, Indiana, Massachusetts, North Carolina, New York, Ohio, Vermont	2,389	1,449	319	319,076	142,278	30.0
2003	Connecticut, Indiana, Massachusetts, North Carolina, New York, Ohio, Vermont	2,441	1,418	261	386,876	168,107	30.5
2004	Indiana, North Carolina, New York, Vermont	1,315	846	176	317,596	135,275	35.0***
All Campaigns	California, Connecticut, Indiana, Massachusetts, North Carolina, New York, Ohio, Vermont	16,977	4,248	635	309,551	132,703	29.0

*** Significant at the 0.01 level.

Rank sum compares yield for campaigns in a given year with yield from campaigns in the previous year.

Similarly, telemarketers may report state campaign figures to one state and national campaign figures to another state. To reduce the chances of double counting proceeds from a single campaign reported in different state reports, we retain only the largest telemarketing campaign completed by a given solicitor for a given nonprofit in any particular year in our dataset. This eliminates 3,551 campaigns, reducing our sample to 13,426 unique telemarketing campaigns known as the firm-fundraiser-year sample (see Table 2).¹³

The next step is to consolidate all of the campaigns on behalf of a given nonprofit in a single year so that we can compare the proceeds from these campaigns to Form 990. To do this, we sum all the campaigns in one year for a unique nonprofit organization from all telemarketing firms (using only the largest campaign reported by each fundraiser). After we total the highest gross receipts from each telemarketing firm associated with a particular nonprofit-year, we have a sample size of 11,375 firm-years (see Table 2). We then compare these firm-year telemarketing proceeds data to the financial reports filed by the nonprofit organizations.

Form 990 Financial Statement Data

For the financial data filed by the nonprofit organizations, we use two sources: the Core files of IRS Form 990s for the years 1994–2004 and the Digitized Data file of IRS Form 990s for the years 1998–2003. Both data sources are distributed to academic researchers by the National Center for Charitable Statistics (NCCS) at the Urban Institute.

The Core data files provide limited information from all tax-exempt organizations required to file a Form 990 or 990-EZ.¹⁴ The Core data is composed of two separate files each year for 501(c)(3) and non-501(c)(3) filers. Both Core files contain the same Form 990 variables, representing the major financial accounts, but exclude some of our variables of interest. In contrast, the NCCS Digitized Data file contains all numbered items from Form 990 and 990-EZ (though not the details from required attached schedules), and thus is more comprehensive than the Core files. However, it only covers 501(c)(3) filers and the later years of our sample.

We use the two data sources to develop two initial datasets. The base dataset employs financial information from both sources. In the early years, this dataset uses the Core data, and, when possible, switches to the Digitized Data file in the later years. As the Digitized Data file does not include non-501(c)(3) organizations, the Core data is used for those firms in the more recent years. The advantage of this “both sources” dataset is that it has the most firm-year observations, allowing us to develop the largest possible database—4,871 firm-years (see Table 2). Unfortunately, the inclusion of the Core data limits our ability to include some variables (*ACCRUAL*, *PROFACCT*, and *RESTRICTED*) due to missing data fields. Additionally, the Core data file includes total contributions, but does not separately include direct contributions (which is where telemarketing gross receipts should be reported) and indirect revenues. For this reason, we develop a second “digitized dataset” that uses only observations from the Digitized Data file. The extensive coding in the Digitized Data file allows the inclusion of numerous additional independent variables (*ACCRUAL*,

¹³ The possibility exists that a nonprofit organization could have multiple campaigns by a given fundraising firm in a single year, so using only the largest campaign will possibly understate donations raised by professional firms. This may reduce the likelihood that we properly identify net reporters, and biases us against finding significant results in our tests.

¹⁴ We use the IRS Core files instead of the Statistics of Income (SOI) files, because the SOI files contain primarily large organizations (assets greater than \$10 million). Many nonprofit organizations that use professional fundraisers are too small to be included in the IRS SOI files.

TABLE 2
Sample Selection

Sample	Level of Observation	Number of Campaigns	Number of Charities	Average Gross Receipts	Average Proceeds to Nonprofits
"Initial Sample" of Telemarketing Campaigns	Campaign	20,203	4,248	\$325,723	\$140,798
"Campaign Level Sample" with Duplicate Campaigns Removed across States and Years	Campaign	16,977	4,248	\$309,551	\$132,703
"Firm-Fundraiser-Year Sample" with Only the Largest Campaigns per Firm-Fundraiser-Year Retained	Firm-Fundraiser-Year	13,426	4,248	\$324,983	\$136,636
"Firm-Year Sample" with Largest Campaigns Summed for Each Firm (not yet matched to financial data)	Firm-Year	11,375	4,248	\$383,580	\$161,273
"Matched Firm-Year Sample" with Largest Campaigns Summed for Each Firm Matched to Financial Data (Core and Digitized Data files)	Firm-Year	4,871	1,625	\$582,543	\$253,404

PROFACCT, and *RESTRICTED*), as well as the more precise direct contributions measurement for comparison with telemarketing reports.

Combined Data

A major challenge is to match the telemarketing campaign data to Form 990 data, since the state campaign data reports identify nonprofits by name (and not by numeric identifier). We employ a multiple-round procedure that involves having research assistants trace the nonprofit name from the campaign reports into the IRS Business Master File, which is a database of organizations granted federal exemption by the IRS, or the GuideStar database of recent Form 990s, in order to identify the associated EIN.¹⁵ After the list of potential matches is developed, the authors review the work and then open and read a number of Form 990s to assure that the name and EIN matches identify the appropriate nonprofit.¹⁶

We are unable to find EINs for some organizations as the IRS Business Master File and GuideStar do not include filings for all tax-exempt organizations. An IRS (1994) study reveals that the Business Master File is missing a significant number of nonprofit organizations. In addition, if police and fire support organizations and public school booster clubs are considered part of their respective municipalities and benefit from municipal government tax exemptions, then they may opt not to file Form 990. In other instances, we find too many possible matches, particularly from federated organizations that have multiple chapters that are independently incorporated nonprofits. If we do not feel confident that we have identified the correct EIN for a nonprofit name, then we do not include the telemarketing observations for that nonprofit in our final datasets.

The next step is to match the data from the state fundraising reports to the datasets we develop using the Core and Digitized Data files. For the “both sources” dataset, we find financial information for 4,871 (43 percent) of the 11,375 observations in our firm-year sample. We find an EIN but are unable to find any financial information for an additional 1,462 (13 percent) of the firm-years. The major reason for these missing observations is that all churches and other nonprofits with less than \$25,000 in annual revenue are exempt from the annual Form 990 filing requirement (see Table 3). Additionally, we find 1,024 firm-year observations in which the combined campaigns had gross receipts of \$25,000 or more in revenues but for which we find no associated Form 990. This indicates that the use of net revenue reporting of telemarketing campaigns may allow nonprofits to avoid filing Form 990s.

The 4,871 firm-year observations with clearly identified EINs represent 8,796 campaigns with average net proceeds of \$171,904 and a median yield of 30 percent. The remaining 6,504 firm-year observations that are not matched to financial data are based on 8,181 campaigns, which generate \$90,554 on average, with a median yield of 26 percent.

Due to the high percentage of telemarketing data that we cannot match to Form 990s and the significant difference in median yield, we are concerned about the generalizability of our results to the broader telemarketing nonprofit population. To assess sample selection bias, we use the campaign sample ($n = 16,977$) and regress the campaign yield (net proceeds remitted to the nonprofit divided by gross proceeds raised on the nonprofit's behalf) on factors found to be relevant in Keating et al. (2003), such as campaign size, the solicitors'

¹⁵ The Core and Digitized Data files identify nonprofit organizations by EIN.

¹⁶ Selected state regulators presently require telemarketers to report the proceeds of telemarketing campaigns. However, there is no easy way for the public to relate this information to the applicable nonprofit as many nonprofit names are similar or related through a federation. The state charity office reports could encourage more public scrutiny if state reports included EINs and the exact name and address of each nonprofit, and ensured that the numbers reported were statewide rather than nationwide figures.

TABLE 3
Matching EINs to Both Sources Dataset

	Number of Firm- Years	Percentage of Firm- Years	Underlying Campaigns	Percentage of Campaigns	Average Net Proceeds to Charity
All Firm-Years	11,375	100%	16,977	100%	\$132,703
Unable to Match EIN to Financial Data because:					
No Form 990 data available	1,462	13%	1,870	11%	
Multiple EINs found	480	4%	1,162	7%	
No EIN found	4,562	40%	5,149	30%	
Total Unmatched	6,504	57%	8,181	48%	\$90,554
Financial Data Found	4,871	43%	8,796	52%	\$171,904

The "Both Sources" dataset includes financial information obtained from both the IRS Core files for 1994–2004 and the NCCS Digitized Data files from 1998–2003.

and nonprofits' experience in telemarketing, and purported charity focus. Our primary variable of interest is whether a campaign can be matched to financial data. We find that this coefficient is insignificant, suggesting that the campaigns that successfully match to Form 990 data do not differ from the unmatched campaigns from a yield perspective after controlling for size, experience, and industry.

V. EMPIRICAL RESULTS

Descriptive Statistics

Using the "both sources" dataset, we have 4,871 firm-years available for analysis. Due to data limitations, we exclude 736 observations from Form 990EZ filers, which lack fundraising expense information, and an additional 61 observations with missing data. Due to problems of data accuracy arising from preparer errors and coding mistakes, we conduct outlier analysis, using the DBETA overall measure of influence, and drop 11 observations that are overly influential in one or both of the regressions. Our final sample is 4,063 firm-year observations representing 1,382 nonprofit organizations. Table 4 provides numerical derivation for the regressions based on the two different datasets.

The means and medians for the various samples are in Table 5. As shown in Panel A, gross reporters differ substantially from the net reporters.¹⁷ The net reporters represent 27 percent of the sample and are more likely to lack accounting sophistication or knowledge as measured by size, officers' compensation, use of accrual accounting, and existence of restricted net assets. The net reporters' median total assets equal \$255,000 as compared to \$3.5 million for gross reporters. Only 52 percent of the net reporters use accrual accounting versus 92 percent for gross reporters, while one-quarter of net reporters report either permanently or temporarily restricted net assets in contrast to gross reporters, 75 percent of which have restricted net assets. Net and gross reporters do not differ significantly in their reliance on external accountants; however net reporters are less likely to be audited than their gross reporting counterparts (56 percent to 96 percent, respectively, in the "digitized dataset"). Net reporters are less likely to be a 501(c)(3) organization (56 percent of net

¹⁷ During the sample period, 91 percent of sample organizations are consistently labeled as either gross or net reporters. Only 3 percent of the organizations have more than one change between reporting categories.

TABLE 4
Sample Selection

	<u>Both Sources</u>	<u>Digitized Dataset</u>
Panel A: Dependent Variable = $P(\text{Net Reporter})$		
Total Firm-Years	4,871	2,648
Less: 990-EZ Filers ^a	(736)	(130)
Observations with Missing Variables	(61)	(38)
Outliers ^b	(11)	(12)
Final Regression Sample	4,063	2,468
Panel B: Dependent Variable = $P(\text{Misclassifier})^c$ Dependent Variable = $P(\text{Allocator})^c$		
Firm-Years in Panel A Final Regression Sample	4,063	2,468
Less: Net Reporters	(1,109)	(585)
Final Regression Sample	2,954	1,883

^a The 990-EZ does not report fundraising expense, which makes all EZ filers appear to be net reporters in the revenue test (fundraising expense appears to equal zero). Additionally, fundraising expense is necessary for the allocator test.

^b Outliers are defined using the DBETA overall measure of influence.

^c The sample begins with firm-years that are labeled gross reporters.

reporters in the “both sources” dataset are 501(c)(3) organizations and 92 percent of gross reporters are 501(c)(3) organizations). Net and gross reporters do not consistently differ significantly in their reliance on debt and the amount retained by professional fundraisers.

Table 5, Panel B presents the means and medians for gross reporters that properly classify telemarketing costs as professional fundraising fees, and those that appear to misclassify their expenses on their Form 990s. Approximately 65 percent of the sample does not properly classify the costs of their telemarketing campaigns as professional fees. Expense misclassifiers are larger (\$3.5 million versus \$3.2 million in median total assets), but pay slightly less to their officers (\$96,000 as compared to \$109,000 in median pay). Misclassifiers use external accountants less often (66 percent compared with 81 percent for proper expense classifiers), but are more likely to use accrual accounting (93 percent versus 89 percent for proper expense classifiers), to have restricted net assets (79 percent versus 68 percent for proper expense classifiers) or to be 501(c)(3) organizations (93 percent compared to 88 percent for proper expense classifiers). There are no significant differences between expense misclassifiers and proper classifiers in the frequency of audits, but proper expense classifiers have significantly more leveraged debt than misclassifiers. Campaign yields tend to be lower for organizations that properly report professional fundraising fees (33 percent versus 38 percent for misclassifiers in the “both sources” dataset and 34 percent versus 40 percent in the “digitized dataset”).

Table 5, Panel C summarizes the means and medians for gross reporters that allocate fundraising expenses and those that do not. Of the 2,954 firm-years in this “both sources” sample (representing 980 nonprofits), 359 firm-years (12 percent) appear to allocate a portion of their telemarketing fees to non-fundraising categories. The allocators are significantly different from non-allocators on most attributes other than 501(c)(3) status. Allocators are smaller (allocator median total assets equal \$394,000 and non-allocator median total assets equal \$4.7 million) and pay their officers much less money (\$11,000 versus

TABLE 5
Descriptive Statistics

Panel A: Gross versus Net Revenue Reporters

	Both Sources		Digitized Dataset	
	Gross	Net	Gross	Net
<i>lnASSETS</i>	14.91 (15.06)	12.42*** (12.45)***	15.23 (15.43)	12.39*** (12.10)***
<i>Assets</i>	5,790,000 (3,500,000)	1,490,000*** (255,000)***	5,280,000 (3,600,000)	1,440,000*** (158,000)***
<i>lnOFFSAL</i>	8.96 (11.51)	4.37*** (0.00)***	9.44 (11.78)	4.14*** (0.00)***
<i>Officers' Salaries</i>	278,538 (100,000)	49,333*** (0.00)***	318,043 (131,148)	67,881*** (0.00)***
<i>ACCRUAL</i>			0.92 (1.00)	0.52*** (1.00)***
<i>PROFACCT</i>			0.71 (1.00)	0.72 (1.00)
<i>RESTRICTED</i>			0.75 (1.00)	0.25*** (0.00)***
<i>501C3</i>	0.92 (1.00)	0.56*** (1.00)***		
<i>AUDIT</i>	0.97 (1.00)	0.71*** (1.00)***	0.96 (1.00)	0.56*** (1.00)***
<i>LEVERAGE</i>	0.49 (0.19)	0.41 (0.02)***	0.54 (0.18)	0.49 (0.08)***
<i>YIELD</i>	36.22 (38.75)	33.72** (28.28)***	37.78 (41.08)	35.85 (29.74)***
<i>n</i>	2,954	1,109	1,883	585

Panel B: Proper Expense Classifiers versus Misclassifiers

	Both Sources		Digitized Dataset	
	Proper Classifier	Misclassifier	Proper Classifier	Misclassifier
<i>lnASSETS</i>	14.61 (14.97)	15.09*** (15.08)***	14.91 (15.40)	15.40*** (15.44)**
<i>Assets</i>	4,190,000 (3,163,000)	6,690,000*** (3,532,689)***	3,900,000 (3,697,000)	6,020,000** (3,598,000)**
<i>lnOFFSAL</i>	9.44 (11.60)	8.68*** (11.48)***	9.77 (11.84)	9.27** (11.75)*
<i>Officers' Salaries</i>	306,593 (108,879)	262,744** (96,322)***	338,314 (138,748)	307,205 (127,002)*
<i>ACCRUAL</i>			0.89 (1.00)	0.93*** (1.00)***
<i>PROFACCT</i>			0.81 (1.00)	0.66*** (1.00)***

(continued on next page)

TABLE 5 (continued)

	Both Sources		Digitized Dataset	
	Proper Classifier	Misclassifier	Proper Classifier	Misclassifier
<i>RESTRICTED</i>			0.68 (1.00)	0.79*** (1.00)***
<i>501C3</i>	0.88 (1.00)	0.93*** (1.00)***		
<i>AUDIT</i>	0.97 (1.00)	0.97 (1.00)	0.95 (1.00)	0.96 (1.00)
<i>LEVERAGE</i>	0.66 (0.18)	0.39** (0.19)	0.67 (0.18)	0.48 (0.18)
<i>YIELD</i>	32.65 (31.85)	38.23*** (43.18)***	33.54 (32.51)	40.05*** (45.38)***
n	1,064	1,890	656	1,227

Panel C: Non-Allocators versus Allocators

	Both Sources		Digitized Dataset	
	Non-Allocator	Allocator	Non-Allocator	Allocator
<i>lnASSETS</i>	15.17 (15.35)	13.07*** (12.88)***	15.50 (15.73)	12.83*** (12.63)**
<i>Assets</i>	6,440,000 (4,656,000)	1,070,000*** (394,000)***	5,820,000 (4,900,000)	4,170,000 (222,000)
<i>lnOFFSAL</i>	9.37 (11.77)	5.99*** (9.29)***	9.80 (11.94)	6.23*** (9.80)***
<i>Officers' Salaries</i>	307,388 (128,728)	69,997*** (10,817)***	345,615 (152,833)	69,454 (18,000)
<i>ACCRUAL</i>			0.93 (1.00)	0.79*** (1.00)***
<i>PROFACCT</i>			0.71 (1.00)	0.78** (1.00)**
<i>RESTRICTED</i>			0.79 (1.00)	0.40*** (0.00)***
<i>501C3</i>	0.92 (1.00)	0.89 (1.00)		
<i>AUDIT</i>	0.97 (1.00)	0.94*** (1.00)***	0.97 (1.00)	0.87*** (1.00)***
<i>LEVERAGE</i>	0.51 (0.19)	0.37** (0.18)**	0.57 (0.18)	0.33** (0.18)**
<i>YIELD</i>	36.62 (40.06)	33.35** (30.63)***	38.17 (42.51)	34.24* (30.00)***
n	2,595	359	1,695	188

***, ** Differences are significant at the 0.10, 0.05, and 0.01 levels, respectively.
Figures reported are means (medians). The significance tests for the means are t-tests and rank sum tests for medians.

\$129,000 in median pay). They also use accrual accounting less frequently (79 percent to 93 percent) and are less likely to report restricted net assets (40 percent compared to 79 percent). Allocators are less likely to be audited (94 percent compared to 97 percent).

Finally, allocators have smaller campaign yields (33 percent versus 37 percent in the “both sources” dataset).

Regression Results

The correlations among the independent variables are presented in Table 6. For the “both sources” full sample, the independent variables with the highest degrees of correlation are *lnASSETS* with *lnOFFSAL* and *501C3*, with correlations of 0.42 and 0.33, respectively. For the more limited sample used in the second regression, the correlations are generally much lower, although a high correlation of 0.43 remains between *lnASSETS* and *lnOFFSAL*.

In the “digitized dataset,” several variables have relatively high correlations, making it less likely that these variables will be significant in regression. The *RESTRICTED*, *ACCRUAL*, *lnASSETS*, *lnOFFSAL*, and *AUDIT* are all correlated with each other in the 0.39 to 0.67 range. The correlations are lower in the more limited sample used in the second regression with most of the correlations of the five variables in the 0.15 to 0.44 range. The correlation of *RESTRICTED* to *lnASSETS*, however, is 0.67.

The first model reported in Tables 7 through 9 provides the results for regressions that use Core and Digitized Data in the “both sources” dataset; the second model in each table reports the regression results for the “digitized dataset” only. Recall that we use both these datasets because some of the variables of interest are not included in the Core data files. However, the “digitized dataset” does not include non-501(c)(3) nonprofits or 1995–1997 Form 990 returns. We focus the analysis first on the larger sample from the “both sources” dataset before turning to the additional insights that can be gleaned from the richer data fields available in the more limited sample from the “digitized dataset.” To better understand the effects of each variable, we focus the discussion on the marginal effects rather than the coefficients.

Table 7 reports the results of logistic regression based on Equation (1), which examines the likelihood of a nonprofit reporting only the net proceeds received from a professional fundraiser as revenue rather than properly reporting the gross proceeds collected. When organizations report on a net basis, they presumably do not report the expense associated with the telemarketing campaign, thus understating reported fundraising expenses. A positive (negative) coefficient in this regression indicates that the organization is more (less) likely to misreport telemarketing activities by reporting only contributions received from the professional fundraiser (i.e., net proceeds).

We start by examining the larger “both sources” sample using the more limited variables available in that dataset. The pseudo R^2 for this model is 29 percent. The estimates on *lnASSETS* and *lnOFFSAL*, which examine the effects of size and professional management, are both negative and statistically significant. This indicates larger organizations and organizations that pay more in total executive salaries are less likely to be classified as misreporting telemarketing activities. A one-unit increase in the natural log of size or officer salaries translates into a 4.2 and 1.1 percentage point decrease, respectively, in the probability of misreporting revenues when other independent variables are held fixed at their means.

Similarly, the governance variables are associated with a reduced likelihood of revenue misreporting. Organizations registered as 501(c)(3) entities are associated with a 33.8 percentage point increase in the likelihood of proper revenue reporting compared with non-501(c)(3) entities. Being audited by a CPA firm is associated with a 34.3 percentage point increase in the chances of being classified as properly reporting telemarketing proceeds.

TABLE 6
Correlations among Independent Variables

Panel A: Independent Variables Used in Logistic Regressions with Both Sources Dataset (n = 4,063)									
	<u>lnASSETS</u>	<u>lnOFFSAL</u>	<u>501C3</u>	<u>AUDIT</u>	<u>LEVERAGE</u>	<u>YIELD</u>			
<u>lnASSETS</u>	1.00								
<u>lnOFFSAL</u>	0.42	1.00							
<u>501C3</u>	0.33	0.27	1.00						
<u>AUDIT</u>	0.25	0.27	0.30	1.00					
<u>LEVERAGE</u>	-0.14	0.02	0.02	-0.01	1.00				
<u>YIELD</u>	0.11	0.01	0.09	0.00	-0.03	1.00			
Panel B: Independent Variables Used in Logistic Regressions with Digitized Dataset (n = 2,468)									
	<u>lnASSETS</u>	<u>lnOFFSAL</u>	<u>ACCRUAL</u>	<u>PROFACCT</u>	<u>RESTRICTED</u>	<u>AUDIT</u>	<u>LEVERAGE</u>	<u>YIELD</u>	
<u>lnASSETS</u>	1.00								
<u>lnOFFSAL</u>	0.43	1.00							
<u>ACCRUAL</u>	0.48	0.40	1.00						
<u>PROFACCT</u>	0.02	0.10	-0.00	1.00					
<u>RESTRICTED</u>	0.67	0.39	0.48	-0.04	1.00				
<u>AUDIT</u>	0.44	0.39	0.54	0.01	0.41	1.00			
<u>LEVERAGE</u>	-0.15	0.03	0.02	-0.01	-0.08	0.01	1.00		
<u>YIELD</u>	0.10	0.01	0.04	-0.03	0.09	-0.00	-0.03	1.00	

TABLE 7
Logistic Regression of the Likelihood of Net Reporting Using Two Datasets

$$P(\text{Net Reporting}) = \beta_0 + \beta_1 \ln \text{ASSETS} + \beta_2 \ln \text{OFFSAL} + \beta_3 \text{ACCRUAL} + \beta_4 \text{PROFACCT} \\ + \beta_5 \text{RESTRICTED} + \beta_6 \text{501C3} + \beta_7 \text{AUDIT} + \beta_8 \text{LEVERAGE} \\ + \beta_9 \text{YIELD} + \beta_{10} \text{SECTOR} + \varepsilon$$

	Predicted Sign	Both Sources		Digitized Dataset	
		Coefficient ^a (Standard Error)	Marginal Effect ^b	Coefficient ^a (Standard Error)	Marginal Effect ^b
<i>lnASSETS</i>	—	−0.26*** (0.03)	−4.2%	−0.10** (0.05)	−1.4%
<i>lnOFFSAL</i>	?	−0.07*** (0.01)	−1.1%	−0.08*** (0.02)	−1.1%
<i>ACCRUAL</i>	—			−0.55** (0.23)	−8.7%
<i>PROFACCT</i>	—			0.05 (0.18)	1.0%
<i>RESTRICTED</i>	—			−0.99*** (0.21)	−15.1%
<i>501C3</i>	—	−1.65*** (0.23)	−33.8%		
<i>AUDIT</i>	—	−1.61*** (0.20)	−34.3%	−1.49*** (0.24)	−28.3%
<i>LEVERAGE</i>	?	−0.06*** (0.02)	−0.1%	−0.02* (0.01)	−0.3%
<i>YIELD</i>	—	0.00 (0.00)	0.0%	0.00 (0.00)	0.0%
Constant		5.35*** (0.47)		2.51*** (0.61)	
n		4,063		2,468	
Pseudo R ²		29%		31%	

*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, two-tailed test.

^a The standard errors and associated z-statistics are adjusted for the intrafirm correlations arising from the inclusion of multiple years of observations (Huber 1967; Rogers 1993).

^b The marginal effect indicates the effect on the probability of net reporting of a one-unit change for continuous variables and a change from 0 to 1 for discrete variables when independent variables are evaluated at their means.

P(Net Reporting) = 1 if *REVDIFF* < 0 or if fundraising expense = 0, and 0 otherwise;

lnASSETS = natural logarithm of beginning assets;

lnOFFSAL = natural logarithm of officers' salaries and wages;

ACCRUAL = 1 if organization uses accrual accounting and 0 if it uses the cash basis;

PROFACCT = 1 if accounting fees > 0, and 0 otherwise;

RESTRICTED = 1 if permanent or temporarily restricted net assets > 0, and 0 otherwise;

501C3 = 1 if entity is organized as a 501(c)(3), and 0 otherwise;

AUDIT = 1 if entity meets the audit requirements of a state in which it operates or solicits funds or is subject to an A-133 audit, and 0 otherwise;

LEVERAGE = total beginning liabilities/total beginning assets; and

YIELD = combined net proceeds from telemarketing campaigns/combined gross proceeds from telemarketing campaigns.

The financial health indicator, however, explains relatively little of the variance in the revenue reporting decision. Highly leveraged organizations are less likely to be classified as misreporting their telemarketing revenues net of expenses. The coefficient on *LEVERAGE* is negative and statistically significant, and a one-unit change would alter the probability of net reporting by 0.1 percentage points. This may indicate that organizations with more debt are monitored by their creditors. The coefficient on the *YIELD* variable is insignificant, suggesting that nonprofits with lower telemarketing yields are not any more likely to be classified as a misreporter than their counterparts.

The “digitized dataset” allows a broader range of variables to be tested and compares telemarketing proceeds to direct contributions, but is limited to 501(c)(3) organizations and to more recent years. The logistic regression model has a pseudo R^2 of 31 percent. The coefficients of the common variables (*lnASSETS*, *lnOFFSAL*, *AUDIT*, *LEVERAGE*, and *YIELD*) are qualitatively similar. The marginal effect of a unit change decrease in *lnASSETS* and *AUDIT* are more modest at 1.4 and 28.3 percentage points, respectively, in the “digitized dataset” as compared to the “both sources” dataset. The statistical significance of the *LEVERAGE* variable is lower although a one-unit change in the variable is modestly higher in the “digitized dataset.”¹⁸

Two of the measures of accounting sophistication, *ACCRUAL* and *RESTRICTED*, are significant in the predicted direction. The use of accrual accounting is associated with an increase in the probability of properly reporting revenues of 8.7 percentage points. Nonprofits with some restricted net assets are 15.1 percentage points more likely to be classified as properly reporting gross revenues from telemarketing campaigns. These results are consistent with the hypothesis that accounting sophistication and knowledge are associated with the likelihood that an organization will misreport revenues and expenses associated with telemarketing campaigns.

Counter to our expectations, the *PROFACCT* variable is insignificant. Krishnan et al. (2006) suggest that *PROFACCT* may proxy for either the use of an outside accountant to assist with preparing Form 990s (or providing other accounting services) or the use of an independent auditor. We specifically control for the use of an independent auditor, include non-501(c)(3) organizations, and use different control variables, all of which may explain the difference in our result from that observed by Krishnan et al. (2006). These results could also differ due to sample variations; nonprofits that employ telemarketers may inherently differ from the large organizations used in their study.

To determine the possible effect that net misreporting has on reported ratios, we examine the organizations that are designated as net reporters because they have fundraising expense equal to zero. We calculate an estimate of the ratio of fundraising expenses to total expenses by increasing reported fundraising expenses (and thus total expenses) by the amount of the telemarketers’ fees. The median ratio of fundraising expense as a percentage of total expense increases from zero to 15.0 percent in the “both sources” dataset and to 11.7 percent in the “digitized dataset” (results untabulated). Based on our estimation of “corrected” fundraising expense, the difference in the ratios is substantial.

The second test examines the factors associated with classifying costs associated with telemarketing campaigns in an expense category other than professional fundraising fees.

¹⁸ We explore whether the negative association between *LEVERAGE* and net reporting is explained by bondholders and bankers either requiring audits or engaging in additional monitoring. In the “digitized dataset,” liabilities can be decomposed between bonds and other liabilities. The sample, however, has relatively few that have outstanding bonds, so that we are not able to effectively evaluate the role of credit relations in promoting better financial reporting practices.

The results of our examination using the logistic regression in Equation (2) are summarized in Table 8. The model has very low explanatory power, with a pseudo R^2 of only 2 percent using the “both sources” dataset and 4 percent using the “digitized dataset.” Very few of the independent variables are statistically significant in the predicted direction.

In the “both sources” dataset, only officers’ salaries and the 501(c)(3) status have a statistically significant relationship to misclassification. Nonprofit organizations that pay relatively higher salaries to their officers are more likely to properly classify their telemarketing costs. A one-unit increase in *lnOFFSAL* is associated with a 1-percentage point increase in the likelihood of proper classification. Nonprofits that are 501(c)(3)s are 18.7 percent more likely to misclassify, which is not consistent with better governance.

The results differ in the “digitized dataset” that focuses exclusively on 501(c)(3) organizations and recent years. The *lnOFFSAL* is not significant, while *PROFACCT* and *RESTRICTED* are. Robustness checks reveal that *lnOFFSAL* is negatively and significantly associated with expense misclassification in the absence of *PROFACCT* but not *RESTRICTED*, suggesting that the significance of *lnOFFSAL* in the both sources regression is due to an omitted correlated variable (*PROFACCT*). In the digitized sample, organizations that use external accountants to assist them with accounting and reporting are significantly less likely to misclassify professional fundraising fees. *PROFACCT*, which measures sophistication acquired from outside the organization, is associated with a 15.9 percentage decrease in the likelihood of misclassification.

An unexpected result is that nonprofits that have restricted net assets are 9.1 percentage points more likely to misclassify telemarketing costs. A possible explanation for this result is that those nonprofits that receive restricted donations (which, in turn, results in restricted assets) are concerned with revealing to their donors that they use an external professional fundraiser.

Overall, the results reported in Table 8 suggest that monitoring does not change the likelihood that telemarketing fees will be properly classified. Additionally, sophistication seems to play a relatively small role, with the use of external accountants being the most significant explanatory factor.

In the third test, the characteristics associated with the telemarketing cost allocation decision are assessed. The samples are limited to the nonprofits designated as gross reporters. Table 9 reports the results from estimating the logistic regression in Equation (3). The pseudo R^2 for the allocation decision is 13 percent using the “both sources” dataset and 19 percent for the “digitized dataset” sample. Both management sophistication variables, *lnASSETS* and *lnOFFSAL*, are negative and statistically significant, meaning that larger nonprofits and those with larger or better compensated professional staffs are less likely to report a portion of the telemarketing fees as program or administrative expenses instead of as fundraising expenses. For the “both sources” regression, a one-unit change in *lnASSETS* and *lnOFFSAL* alters the probability of allocating costs by 1.8 and 0.6 percentage points, respectively. A potential explanation for this result is that nonprofits that are relatively smaller in size or in terms of their officers’ payroll manage telemarketing costs to report more favorable fundraising (or program) ratios.

Governance and accounting sophistication variables perform quite differently in the allocation and revenue recognition decisions. In the “both sources” dataset, the 501(c)(3) organization variable is marginally significant, but in the opposite of the predicted direction. It increases the likelihood of allocation of telemarketing costs by only 3.5 percentage points as compared to a decrease of 33.8 percentage point effect in the gross revenue test. The audit variable is insignificant in both allocation regressions, whereas it has a substantial mitigating effect in the net reporting regression. This finding is not surprising given that

TABLE 8
Logistic Regression of Likelihood of Misclassification of Professional Fundraising Fee Using Two Datasets

$$P(\text{Misclassifier}) = \gamma_0 + \gamma_1 \ln \text{ASSETS} + \gamma_2 \ln \text{OFFSAL} + \gamma_3 \text{ACCRUAL} + \gamma_4 \text{PROFACCT} \\ + \gamma_5 \text{RESTRICTED} + \gamma_6 \text{501C3} + \gamma_7 \text{AUDIT} + \gamma_8 \text{LEVERAGE} \\ + \gamma_9 \text{YIELD} + \gamma_{10} \text{SECTOR} + \varepsilon$$

	Predicted Sign	Both Sources		Digitized Dataset	
		Coefficient ^a (Standard Error)	Marginal Effect ^b	Coefficient ^a (Standard Error)	Marginal Effect ^b
<i>lnASSETS</i>	—	0.05 (0.03)	1.1%	0.01 (0.04)	0.2%
<i>lnOFFSAL</i>	?	−0.04*** (0.01)	−1.0%	−0.03 (0.02)	−0.6%
<i>ACCRUAL</i>	—			0.25 (0.27)	5.8%
<i>PROFACCT</i>	—			−0.76*** (0.18)	−15.9%
<i>RESTRICTED</i>	—			0.40** (0.20)	9.1%
<i>501C3</i>	—	0.77*** (0.24)	18.7%		
<i>AUDIT</i>	—	−0.10 (0.25)	−2.3%	0.02 (0.35)	0.5%
<i>LEVERAGE</i>	?	−0.03 (0.02)	−0.1%	−0.00 (0.02)	−0.1%
<i>YIELD</i>	—	0.00 (0.00)	0.0%	0.00* (0.00)	0.0%
Constant		−0.28 (0.40)		0.65 (0.51)	
n		2,954		1,883	
Pseudo R ²		2%		4%	

*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, two-tailed test.

^a The standard errors and associated z-statistics are adjusted for the intrafirm correlations arising from the inclusion of multiple years of observations (Huber 1967; Rogers 1993).

^b The marginal effect indicates the effect on the probability of net reporting of a one-unit change for continuous variables and a change from 0 to 1 for discrete variables when independent variables are evaluated at their means.

$P(\text{Misclassifier}) = 1$ if $\text{FEEDIFF} < 0$, and 0 otherwise;

lnASSETS = natural logarithm of beginning assets;

lnOFFSAL = natural logarithm of officers' salaries and wages;

ACCRUAL = 1 if organization uses accrual accounting, and 0 otherwise;

PROFACCT = 1 if accounting fees > 0, and 0 otherwise;

RESTRICTED = 1 if permanent or temporarily restricted net assets > 0, and 0 otherwise;

501C3 = 1 if entity is organized as a 501(c)(3), and 0 otherwise;

AUDIT = 1 if entity meets the audit requirements of a state in which it operates or solicits funds or is subject to an A-133 audit, and 0 otherwise;

LEVERAGE = total beginning liabilities/total beginning assets; and

YIELD = combined net proceeds from telemarketing campaigns/combined gross proceeds from telemarketing campaigns.

TABLE 4
Changes in PIN Analysis Controlling for Beginning of Change Period PIN
Fama-MacBeth Regressions

Change Period		x = -1		x = 0		x = +1	
		Predicted Sign	Parameter Estimate	p-value	Parameter Estimate	p-value	Parameter Estimate
Panel A: Positive Returns (Good News) Sample							
$NI_t = \alpha + \beta_1 RET_t + \beta_2 \Delta PIN_{t+x} + \beta_3 \Delta PIN_{t+x} * RET_t + \beta_4 PIN_{t-1+x} + \beta_5 PIN_{t-1+x} * RET_t + \varepsilon_t$							
Intercept	?	0.068	0.00	0.064	0.00	0.071	0.00
RET_t	+	0.026	0.19	0.063	0.01	0.060	0.01
ΔPIN_{t+x}	?	0.015	0.68	-0.033	0.42	-0.102	0.02
$\Delta PIN_{t+x} * RET_t$	-	-0.180	0.07	-0.092	0.32	-0.003	0.49
PIN_{t-1+x}	?	-0.032	0.38	-0.021	0.53	-0.053	0.06
$PIN_{t-1+x} * RET_t$	-	0.085	0.28	-0.071	0.24	-0.076	0.19
Average Adj. R ²		0.05		0.06		0.05	
Average n		597		597		597	
Panel B: Negative Returns (Bad News) Sample							
$NI_t = \alpha + \beta_1 RET_t + \beta_2 \Delta PIN_{t+x} + \beta_3 \Delta PIN_{t+x} * RET_t + \beta_4 PIN_{t-1+x} + \beta_5 PIN_{t-1+x} * RET_t + \varepsilon_t$							
Intercept	?	0.076	0.00	0.069	0.00	0.085	0.00
RET_t	+	0.350	0.00	0.210	0.04	0.199	0.08
ΔPIN_{t+x}	?	0.028	0.72	-0.026	0.67	-0.212	0.04
$\Delta PIN_{t+x} * RET_t$	+	0.470	0.09	0.873	0.02	-0.066	0.45
PIN_{t-1+x}	?	-0.069	0.28	-0.043	0.36	-0.128	0.09
$PIN_{t-1+x} * RET_t$	+	0.160	0.37	0.786	0.03	0.811	0.08
Average Adj. R ²		0.16		0.17		0.18	
Average n		343		343		343	
Panel C: Combined Sample							
$NI_t = \alpha + \beta_1 DR_t + \beta_2 RET_t + \beta_3 RET_t * DR_t + \beta_4 \Delta PIN_{t+x} + \beta_5 \Delta PIN_{t+x} * DR_t + \beta_6 \Delta PIN_{t+x} * RET_t + \beta_7 \Delta PIN_{t+x} * RET_t * DR_t + \beta_8 PIN_{t-1+x} + \beta_9 PIN_{t-1+x} * DR_t + \beta_{10} PIN_{t-1+x} * RET_t + \beta_{11} PIN_{t-1+x} * RET_t * DR_t + \varepsilon_t$							
Intercept	?	0.068	0.00	0.064	0.00	0.071	0.00
DR_t	?	0.008	0.68	0.005	0.73	0.015	0.41
RET_t	+	0.026	0.19	0.063	0.01	0.060	0.01
$RET_t * DR_t$	+	0.324	0.01	0.147	0.05	0.139	0.15
ΔPIN_{t+x}	?	0.015	0.68	-0.033	0.42	-0.102	0.02
$\Delta PIN_{t+x} * DR_t$	?	0.013	0.90	0.007	0.93	-0.109	0.16
$\Delta PIN_{t+x} * RET_t$	-	-0.180	0.08	-0.092	0.32	-0.003	0.49
$\Delta PIN_{t+x} * RET_t * DR_t$	+	0.650	0.03	0.965	0.01	-0.063	0.45
PIN_{t-1+x}	?	-0.032	0.38	-0.021	0.53	-0.053	0.06

(continued on next page)

agency cost. The combined results indicate the effect of information asymmetry on conservatism cannot be attributed to other potential sources of conservatism, specifically debt contracts, compensation contracts, and litigation.

Changes in PIN Scores and Conservatism

Table 4 presents the results testing the second and third hypotheses using Equations (4) and (5). The time period and samples used for the Fama-MacBeth regressions are smaller than those used in the previous tables due to the requirement that firms have prior ($t-1$), current (t) and future ($t+1$) changes in PIN scores, specifically we estimate Equations (4) and (5) using an average of 940 observations over the 1985–2000 time period.²²

Panels A and B of Table 4 report the results of estimating Equation (5) for the positive and negative returns samples respectively. We first discuss the coefficients on non- ΔPIN score terms. The RET coefficient is positive in all six specifications and significant in all but one of the specifications, the positive return prior period change in PIN specification. The coefficient on the $PIN_{t-1+x} * RET_t$ interaction term is insignificant in all three of the positive return specifications and positive and significant in two of the three negative return sample estimations.

Consistent with H2a, changes in PIN lead the extent to which gains are reflected in the financial statements, the coefficient on the $\Delta PIN_{t-1} * RET_t$ term is -0.18 , significant at the 0.07 level, one-sided. In both the contemporaneous and future period specifications the coefficient on the $\Delta PIN_{t+x} * RET_t$ term is insignificant. For the negative return sample, the coefficient on the $\Delta PIN_{t+x} * RET_t$ term is positive and significant at the 0.09 level or better, one-sided, for the prior and contemporaneous period change in PIN estimations. These results suggest that as information asymmetries increase more losses are reflected in the current financial statements, indicating that changes in information asymmetry lead the extent to which losses are reflected in earnings, consistent with H2b. In addition, the coefficient on the $\Delta PIN_{t+x} * RET_t$ term for the future period change in PIN specification is insignificant.

Panel C of Table 4 presents the results from estimating Equation (4). In all three estimations the RET coefficient is positive, but only significant at conventional levels for the contemporaneous and future changes in PIN specifications. Similar to the RET term, the $RET * DR$ term is positive in all three estimations, but only significant in the prior and contemporaneous changes in PIN specifications. The differences in the magnitude and significance of the RET and $RET * DR$ terms across the specifications is due to the difference in effects of the beginning of change period PIN scores across specification on contemporaneous conservatism.

The first two columns of Panel C of Table 4 present the results for the ΔPIN_{t-1} specification, where the change in the PIN is measured over the year prior to the conservatism year and the PIN score is as of the beginning of the $t-1$ change period. The coefficient on the $\Delta PIN_{t-1} * RET$ interaction term is negative and significant, at the 0.08 level, one-sided. This result suggests that when information asymmetries between managers and outside equity investors increase, earnings incorporate less good news in the subsequent period. The coefficient on the $\Delta PIN_{t-1} * RET * DR$ (β_7) is 0.650, significant at the 0.03 level, one-sided, indicating that increases in information asymmetry in the previous year are associated

²² Recall that we have PIN scores as of December 31 for the period 1983–2001. In our change test we use the change in the PIN score over the year, limiting our analysis to the 1985 to 2000 time period, i.e., in the first year the lag change in PIN is the 1984 PIN minus 1983 PIN and in our last year the lead change in PIN is the 2001 PIN minus the 2000 PIN.

the previous tests due to the additional data required for Shu's (2000) measure. Consistent with litigation being a determinant of conservatism the coefficient on the $PROBLIT * RET * DR$ interaction term is positive and significant at the 0.01 level or better, one-sided in each of the three specifications.

After controlling for the influence of the market-to-book ratio, debt contracts, and litigation on conservatism, we continue to find that firms with higher PIN scores report more conservative earnings. In each of the three horizons the coefficient on the three-way interaction, $PIN * RET * DR$, is positive and significant at the 0.06 level or better one-sided. The results on the $PIN * RET$ term are more mixed. For the one-year horizon the coefficient on $PIN * RET$ is positive and significant at the 0.08 level, one-sided, inconsistent with our first hypothesis. However, the coefficient on the $PIN * RET$ interaction term is negative in two of the three specifications, only significant, however, at conventional levels in the three-year horizon.

Overall the combined evidence reported in Table 3 is consistent with earnings incorporating less information about gains and more information about losses as information asymmetry increases. The combination of these results indicates that information asymmetries between managers and outside investors are an important determinant of conservatism.¹⁹ The significance of the coefficient of $PIN * RET * DR$ is a strong result given that MB , leverage, and litigation are all associated with information asymmetry. Since PIN is the only direct measure of the asymmetry included in the regression, we interpret the result as confirmation that asymmetry affects conservatism.

As noted earlier, earnings-based compensation contracts may generate a demand for conservatism. We attempt to control for the influence of compensation contracting on conservatism by including $\%OPTIONS$ in Equation (7).²⁰ We expect firms paying their CEOs with relatively more stock options to use relatively less earnings-based compensation. In untabulated analysis, we collect compensation data from S&P's ExecuComp over the 1992 to 2001 time period, reducing the sample sizes to an average of 627 observations per year. We first included only the main terms (DR , RET , and $RET * DR$) and the $\%OPTIONS$ terms in Equation (7). In both the one- and two-year horizons we find a negative and significant coefficient on the three-way interaction, $\%OPTIONS * RET * DR$, at the 0.04 level or better, one-sided. This result is consistent with greater earnings-based compensation increasing the demand for conservatism. When estimating the full model none of the $PIN * RET$ interaction terms are significant in the $\%OPTIONS$ sample. However, the coefficient on the $PIN * RET * DR$ term is positive and significant at the 0.03 level or better, one-sided, in the one-, two-, and three-year horizons.²¹

Overall, the results reported in Tables 2 and 3 lend support to the first hypothesis and indicate that both debt and equity investors demand conservatism as a means of mitigating

¹⁹ The results for the two- and three-year specifications are estimated annually, resulting in overlapping periods. We correct for autocorrelation in the coefficients, using the Newey-West procedure, mitigating concerns that the overlapping periods are driving our results. Nevertheless, we investigate whether our results are robust to using non-overlapping periods for the longer horizon Basu estimates. Specifically, for the two- (three-) year estimations, we repeat the analysis using every second (third) year, eliminating the overlapping periods. The results from this analysis are similar to those reported in the tables.

²⁰ We investigated alternative proxies for the compensation-based demands for conservatism; however, we were able to find little evidence of these alternative variables influencing the Basu coefficient. The evidence indicates that $\%OPTIONS$ is a noisy proxy, at best, for the compensation-based demands for conservatism.

²¹ We repeat the analysis presented in Table 4 with one additional control variable, Altman's (1968) Z-score, and its interaction terms. We include the decile rank of Altman's (1968) Z-score, finding that firms with higher Z-scores (less distressed) report less conservative earnings. More importantly we find that the results on the PIN score are robust to the inclusion of the Z-score.

a strong test for the effect of information asymmetry among equity investors on conservatism.¹⁷

Panel A of Table 3 presents the results from estimating Equation (6). The results document that the *MB* ratio exhibits a negative association with conservatism for all three horizons. Consistent with the findings of Frankel and Roychowdhury (2006), firms with higher leverage report more conservative earnings as evidenced by the positive and significant coefficient on the *LEV * RET * DR* interaction term in each of the horizons.¹⁸ These results are consistent with the existence of debt-contracting-based demands for conservatism. Turning to the *PIN* score interactions, the coefficient on the *PIN * RET* term is negative and significant at the 0.05 level or better, for the two- and three-year horizons. Moreover, the *PIN * RET * DR* interaction term continues to be positive and significant at the 0.05 level or better, one-sided in all three horizons, consistent with H1c.

While debt contracts are the most widely accepted source of conservatism, Beaver (1993) and Watts (1993) suggest that litigation may also be a source of conservatism. Consistent with litigation being a source of conservatism, Basu (1997) finds that earnings are more conservative in periods where auditors' legal liability increases. Shu (2000) develops a measure of auditor litigation risk by examining the firm-specific factors associated with an increase in the likelihood of auditor litigation. We measure auditor litigation risk using the parameters from Shu's (2000) logit model to calculate the probability of auditor litigation for each firm-year observation in our sample. We examine whether our results on the *PIN* score are robust to controlling for the litigation based demands for conservatism using the following equation:

$$\begin{aligned}
 NI = & \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \beta_4 MB + \beta_5 MB * DR \\
 & + \beta_6 MB * RET + \beta_7 MB * RET * DR + \beta_8 LEV + \beta_9 LEV * DR \\
 & + \beta_{10} LEV * RET + \beta_{11} LEV * RET * DR + \beta_{12} PROBLIT \\
 & + \beta_{13} PROBLIT * DR + \beta_{14} PROBLIT * RET \\
 & + \beta_{15} PROBLIT * RET * DR + \beta_{16} PIN + \beta_{17} PIN * DR \\
 & + \beta_{18} PIN * RET + \beta_{19} PIN * RET * DR + \epsilon
 \end{aligned} \tag{7}$$

where *PROBLIT* is the probability of auditor litigation, where the probabilities are calculated using the parameters from Table 3 of Shu (2000). All other variables are as previously defined.

Panel B of Table 3 presents the results from estimating Equation (7). The sample sizes used to estimate Equation (7) are somewhat smaller (average $n = 872$) than those used in

¹⁷ Just as Smith and Watts (1992, 269–270) do not specify the partial effects of different factors on corporate policies or the interdependencies among those factors, we do not specify any interdependencies among or partial effects of the various sources of conservatism. The reason is the same: sorting out these various effects requires specification of a structural model that takes into account these interdependencies and errors in specifying those relations are likely to be large and introduce bias into the empirical analysis. At this stage of the development of the conservatism literature, we believe documentation of robust empirical relations between policy parameters (including conservatism) and exogenous variables (we assume the investment opportunity set is exogenous) is likely to better serve progress in understanding conservatism.

¹⁸ In untabulated test we estimate a model with controls for the positive and negative return samples, respectively. The coefficients on *PIN * RET* terms are negative for the positive returns sample and positive for the negative returns sample, consistent with H1a and H1b. For the negative returns sample the *PIN* interaction is positive and significant at the 0.05 level or better one-side, for all three horizons, whereas for the positive returns sample *PIN* interaction is only significant at conventional levels for the two- and three-year horizons.

TABLE 3 (continued)

Estimation Period	Predicted Sign	One-Year		Two-Year		Three-Year	
		Parameter Estimate	p-value	Parameter Estimate	p-value	Parameter Estimate	p-value
<i>MB * RET</i>	—	−0.003	0.11	−0.012	0.00	−0.015	0.00
<i>MB * RET * DR</i>	−/+	−0.023	0.09	−0.019	0.08	−0.024	0.03
<i>LEV</i>	?	−0.033	0.01	−0.023	0.22	−0.041	0.14
<i>LEV * DR</i>	?	−0.031	0.33	−0.090	0.06	−0.074	0.32
<i>LEV * RET</i>	—	−0.089	0.01	−0.064	0.10	−0.011	0.36
<i>LEV * RET * DR</i>	+	0.497	0.00	0.338	0.00	0.297	0.08
<i>PROBLIT</i>	?	−0.017	0.11	0.004	0.80	0.021	0.54
<i>PROBLIT * DR</i>	?	0.011	0.39	0.011	0.69	−0.029	0.59
<i>PROBLIT * RET</i>	?	0.001	0.49	−0.019	0.17	−0.016	0.30
<i>PROBLIT * RET * DR</i>	+	0.126	0.01	0.236	0.00	0.187	0.00
<i>PIN</i>	?	−0.150	0.00	−0.274	0.00	−0.356	0.01
<i>PIN * DR</i>	?	−0.030	0.61	−0.022	0.85	0.071	0.58
<i>PIN * RET</i>	—	0.110	0.08	−0.011	0.40	−0.134	0.04
<i>PIN * RET * DR</i>	+	0.537	0.06	0.506	0.06	0.637	0.05
Average Adj. R ²		0.26		0.31		0.37	
Average n		872		872		872	

This table presents the results of Fama-MacBeth regressions estimated annually over the 1983 to 2001 time horizon.

p-values are based on the time-series standard errors of the coefficient estimates, where the standard errors are corrected for autocorrelation using the Newey-West procedure. p-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise.

In the one-year specification earnings and returns are measured over the fiscal year. In the two-year specification earnings and returns are aggregated over the current and prior fiscal year. In the three-year specification earnings and returns are aggregated over the current and prior two fiscal years.

NI = cumulative net income before extraordinary items (Compustat #18) divided by beginning of aggregation period market value of equity;

RET = buy and hold return over the aggregation period;

DR = 1 if *RET* is negative, and 0 otherwise;

MB = market-to-book ratio defined as market value of equity (Compustat #199 * Compustat #25) divided by book value of equity (Compustat #60) at the end of the aggregation period;

LEV = total debt (Compustat #9 + Compustat #34) divided by total assets (Compustat #6) at the end of the aggregation period;

PROBLIT = parameter estimates from Table 3 of Shu (2000) applied to the accounting and market measures of the sample firms that have the necessary data to calculate the measures; and

PIN = probability of an information based trade as of the end of the aggregation period.

ratio in conservatism tests. *MB* reflects both net asset conservatism and growth options. We expect the greater a firm's growth options, the greater is the information asymmetry between equity investors in the firm. We use *LEV* to proxy for the debt contract demand for conservatism. Our first test of H1 *controls* for both *MB* and contracting, using *LEV* as the only control for contracting.¹⁶ Note, that like *MB*, *LEV* varies with growth options. Since growth options create information asymmetries, the inclusion of both these variables generates

¹⁶ There are a couple of reasons to believe that control will be reasonable: (1) accounting history is consistent with debt contracting being a major contracting influence on conservatism (Watts 1993, 2006); and (2) the extant evidence is consistent with accounting's inability to reflect growth options driving the reduction in the use of both debt and accounting-based compensation contracts. Consequently, *LEV* could well proxy for both contracting uses of accounting.

TABLE 3
Conservatism and PIN
Controlling for Other Factors Influencing the Demand for Conservatism
Fama-MacBeth Regressions

Estimation Period	One-Year			Two-Year		Three-Year	
	Predicted Sign	Parameter Estimate	p-value	Parameter Estimate	p-value	Parameter Estimate	p-value
Panel A: Combined Sample Market-to-Book and Leverage Controls							
$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \beta_4 MB + \beta_5 MB * DR + \beta_6 MB * RET$ $+ \beta_7 MB * RET * DR + \beta_8 LEV + \beta_9 LEV * DR + \beta_{10} LEV * RET$ $+ \beta_{11} LEV * RET * DR + \beta_{12} PIN + \beta_{13} PIN * DR + \beta_{14} PIN * RET$ $+ \beta_{15} PIN * RET * DR + \varepsilon$							
Intercept	?	0.102	0.00	0.189	0.00	0.278	0.00
DR	?	0.025	0.15	0.043	0.20	0.041	0.16
RET	+	0.089	0.00	0.198	0.00	0.269	0.00
RET * DR	+	0.162	0.10	0.106	0.09	0.165	0.01
MB	?	-0.009	0.00	-0.017	0.00	-0.030	0.00
MB * DR	?	-0.001	0.72	-0.009	0.11	-0.019	0.01
MB * RET	-	-0.002	0.18	-0.012	0.00	-0.022	0.00
MB * RET * DR	-/+	-0.034	0.07	-0.021	0.07	-0.027	0.02
LEV	?	-0.020	0.05	-0.008	0.63	0.005	0.81
LEV * DR	?	-0.047	0.18	-0.099	0.03	-0.109	0.21
LEV * RET	-	-0.107	0.01	-0.066	0.07	-0.024	0.18
LEV * RET * DR	+	0.398	0.00	0.310	0.00	0.252	0.04
PIN	?	-0.101	0.00	-0.213	0.01	-0.273	0.02
PIN * DR	?	-0.064	0.28	-0.052	0.65	0.062	0.61
PIN * RET	-	0.003	0.49	-0.112	0.02	-0.107	0.05
PIN * RET * DR	+	0.583	0.05	0.839	0.02	0.836	0.03
Average Adj. R ²		0.26		0.32		0.47	
Average n		1,073		1,073		1,073	

Panel B: Combined Sample Market-to-Book, Leverage, and Litigation Controls

$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \beta_4 MB + \beta_5 MB * DR + \beta_6 MB * RET$ $+ \beta_7 MB * RET * DR + \beta_8 LEV + \beta_9 LEV * DR + \beta_{10} LEV * RET$ $+ \beta_{11} LEV * RET * DR + \beta_{12} PROBLIT + \beta_{13} PROBLIT * DR$ $+ \beta_{14} PROBLIT * RET + \beta_{15} PROBLIT * RET * DR + \beta_{16} PIN$ $+ \beta_{17} PIN * DR + \beta_{18} PIN * RET + \beta_{19} PIN * RET * DR + \varepsilon$							
Intercept	?	0.111	0.00	0.193	0.00	0.294	0.00
DR	?	0.002	0.89	0.021	0.53	-0.007	0.74
RET	+	0.063	0.02	0.170	0.00	0.236	0.00
RET * DR	+	0.021	0.42	0.044	0.28	0.103	0.09
MB	?	-0.008	0.00	-0.015	0.00	-0.030	0.00
MB * DR	?	0.000	0.97	-0.010	0.05	-0.007	0.14

(continued on next page)

TABLE 2
Conservatism and PIN
Fama-MacBeth Regressions

Estimation Period	One-Year			Two-Year		Three-Year	
	Predicted Sign	Parameter Estimate	p-value	Parameter Estimate	p-value	Parameter Estimate	p-value
Panel A: Positive Return (Good News) Sample							
$NI = \alpha + \beta_1 RET + \beta_2 PIN + \beta_3 PIN * RET + \epsilon$							
Intercept	?	0.073	0.00	0.148	0.00	0.224	0.00
RET	+	0.045	0.05	0.122	0.00	0.167	0.00
PIN	?	-0.054	0.11	-0.124	0.08	-0.184	0.12
PIN * RET	-	-0.032	0.36	-0.115	0.04	-0.136	0.06
Average Adj. R ²		0.03		0.08		0.15	
Average n		672		727		764	
Panel B: Negative Return (Bad News) Sample							
$NI = \alpha + \beta_1 RET + \beta_2 PIN + \beta_3 PIN * RET + \epsilon$							
Intercept	?	0.086	0.00	0.154	0.00	0.238	0.00
RET	+	0.284	0.00	0.343	0.00	0.532	0.00
PIN	?	-0.107	0.06	-0.196	0.13	-0.354	0.02
PIN * RET	+	0.837	0.02	0.892	0.01	0.453	0.10
Average Adj. R ²		0.19		0.19		0.18	
Average n		401		346		309	
Panel C: Combined Sample							
$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \beta_4 PIN + \beta_5 PIN * DR + \beta_6 PIN * RET + \beta_7 PIN * RET * DR + \epsilon$							
Intercept	?	0.073	0.00	0.148	0.00	0.224	0.00
DR	?	0.013	0.28	0.006	0.82	0.014	0.64
RET	+	0.045	0.05	0.122	0.00	0.167	0.00
RET * DR	+	0.238	0.01	0.221	0.00	0.365	0.00
PIN	?	-0.054	0.11	-0.124	0.08	-0.184	0.11
PIN * DR	?	-0.053	0.39	-0.072	0.57	-0.170	0.19
PIN * RET	-	-0.032	0.36	-0.115	0.04	-0.136	0.04
PIN * RET * DR	+	0.868	0.01	1.007	0.01	0.590	0.09
Average Adj. R ²		0.21		0.27		0.32	
Average n		1,073		1,073		1,073	

This table presents the results of Fama-MacBeth regressions estimated annually over the 1983 to 2001 time horizon.

p-values are based on the time-series standard errors of the coefficient estimates, where the standard errors are corrected for autocorrelation using the Newey-West procedure. p-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise.

In the one-year specification earnings and returns are measured over the fiscal year. In the two-year specification earnings and returns are aggregated over the current and prior fiscal year. In the three-year specification earnings and returns are aggregated over the current and prior two fiscal years.

NI = cumulative net income before extraordinary items (Compustat #18) divided by beginning of aggregation period market value of equity;

RET = buy and hold return over the aggregation period;

DR = 1 if RET is negative, and 0 otherwise; and

PIN = probability of an information-based trade as of the end of the aggregation period.

investments generate debt capacity, leverage will also measure the debt contracting demand for conservatism. This helps us distinguish the sources of conservatism in the next section.

V. RESULTS

PIN Scores and Conservatism

Hypothesis 1 predicts that higher PIN scores are associated with greater asymmetric recognition of gains and losses in the current financial statements. Table 2 reports the Fama and MacBeth (1973) results testing our first hypotheses. We test our first hypotheses using three time periods labeled, One-Year, Two-Year, and Three-Year. For the one-year tests we measure earnings and returns over the fiscal year. In the two- (three-) year tests we measure earnings and returns cumulatively over the current and the prior one (two) fiscal years, consistent with the Roychowdhury and Watts (2007).

Panel A of Table 2 estimates Equation (2) for the positive return sample, cumulating earnings and returns over the three time intervals. Consistent with H1a, the higher the PIN score, the less gains reflected in the current financial statements, the coefficient on the $PIN * RET$ term is negative in each of the three horizons. However, the coefficient on the $PIN * RET$ term is only significant in the two- and three-year specifications, at the 0.06 level or better one-sided. Panel B estimates Equation (2) for the negative return sample. Consistent with H1b, the higher the PIN score the more losses reflected in the current financial statements, the coefficient on the $PIN * RET$ interaction term is positive and significant, at the 0.10 level or better one-side, for all three horizons.

Panel C of Table 2 estimates Equation (3). For brevity we do not discuss the coefficient on RET and $RET * DR$, but note that the signs and statistical significance are similar to those found in prior research. The coefficient on the two-way interaction between $PIN * RET$ is insignificant in the one-year horizon specification and negative and significant at the 0.04 level in the two- and three-year specifications, indicating that firms with higher PIN scores incorporate less good news into earnings. Turning to the three-way interaction term, $PIN * RET * DR$, the coefficient is positive and significant at the 0.09 level or better one-sided, for all three horizons. These results indicate that as the level of information asymmetry between insiders and uninformed investors' increases, earnings exhibit greater conservatism, consistent with H1c.

To test the robustness of the relation between information asymmetry between managers and outside equity investors and conservatism, we control for a number of variables known to influence conservatism. Table 3 presents the results from Fama and MacBeth (1973) estimations of the following equation:

$$\begin{aligned}
 NI = & \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \beta_4 MB_t + \beta_5 MB * DR \\
 & + \beta_6 MB * RET + \beta_7 MB * RET * DR + \beta_8 LEV + \beta_9 LEV * DR \\
 & + \beta_{10} LEV * RET + \beta_{11} LEV * RET * DR + \beta_{12} PIN + \beta_{13} PIN * DR \\
 & + \beta_{14} PIN * RET + \beta_{15} PIN * RET * DR + \varepsilon
 \end{aligned} \tag{6}$$

where all variables are as previously defined.

Roychowdhury and Watts (2007) demonstrate the importance, both empirically and theoretically, of controlling for the composition of equity values through the use of the MB

While PIN scores are designed to measure information asymmetry between informed (managers) and uninformed equity investors, it is likely that they are also related to information asymmetries between banks and managers and between corporate boards and managers. As we have argued, growth options affect all three asymmetries. In turn, this suggests that PIN will be related to corporate financial policy (leverage) and management compensation policy (use of earnings-based compensation and stock-price-based compensation). Growth options are also likely to affect dividend policy because they reduce the free cash flow problems associated with assets-in-place (Jensen 1986).¹⁵

We examine the variation in PIN scores with *MB* and corporate policies where data are readily available to us: leverage (*LEV*); dividend yield (*DIVYLD*; (common dividends + repurchases/market value of equity)) and option compensation as a percent of total compensation (*%OPTIONS*, value of options granted to the CEO/total value of compensation paid to the CEO). Leverage is expected to decline with growth options and consequently with the PIN score. Dividend yield is also expected to decline with the PIN score because the greater the growth options, the less cost to restricting dividends and the less the free cash flow problem. Options as a percentage of total compensation is expected to increase with the PIN score because earnings will reflect the effect of the manager's actions less as growth options increase leading to relatively more non-earnings-based compensation. *MB* is expected to vary positively with the PIN score, to the extent that it measures growth options. Firm size is positively correlated with the relative amount of public information which reduces information asymmetry between investors generating a negative association with the PIN.

To provide an overview of the association between *PIN* and the policy variables, we regress *PIN* on the policy variables plus *MB* and *SIZE* (log of the market value of equity). Panel C of Table 1 presents the results for two Fama and MacBeth (1973) regressions estimated over two time periods (1983–2001 and 1992–2001): one including, and one excluding *%OPTIONS* as an independent variable.

All the estimated coefficients of the independent variables in Panel C of Table 1 have the predicted sign and are significant at least at the 0.08 level (one-sided test). The coefficient of *MB* is positive and significant at the 0.07 level (one-sided test). This is consistent with the market-to-book ratio reflecting growth options and the information asymmetries associated with them. The negative sign of the coefficient of *SIZE* is significant at the 0.01 level (one-sided test), consistent with extant empirical evidence that there is relatively more public information for larger firms (Banz 1981). The coefficient on *%OPTIONS* is positive and significant at the 0.08 (one-sided test), consistent with firm relying less on earnings-based compensation as growth options increase. These results combined with the significance and predicted signs on the policy coefficients give us more assurance that *PIN* is measuring information asymmetry.

The negative association between *LEV* and *PIN* likely comes from two sources. First, increases in growth options are not financed by debt, thus reducing leverage (see our earlier discussion about the inability of debt covenants to handle growth options) and, second, those increases reduce the optimal total level of firm debt (Barclay et al. 2006). Leverage proxies for the firm's relative amount of non-growth option investments and since only such

¹⁵ A number of studies have found that corporate financial, dividend, and compensation policies vary with growth options as proxied by the market value of the firm to the book value of the firm (e.g., Smith and Watts 1992; Gaver and Gaver 1993; Barclay et al. 1995). These results suggest that corporate policies are likely to be related to PIN.

TABLE 1 (continued)

Panel C: *PIN* Investment Opportunity Set Variables Fama-MacBeth Regressions

$$PIN = \alpha + \beta_1 SIZE + \beta_2 MB + \beta_3 LEV + \beta_4 DIVYLD + \beta_5 \%OPTIONS + \varepsilon$$

	Predicted Sign	Full Sample		Sample with Compensation Data	
		Parameter Estimate	p-value	Parameter Estimate	p-value
Intercept	?	0.352	0.00	0.295	0.00
SIZE	-	-0.024	0.00	-0.019	0.00
MB	+	0.001	0.00	0.001	0.07
LEV	-	-0.032	0.00	-0.019	0.00
DIVYLD	-	-0.049	0.00	-0.090	0.00
%OPTIONS	+			0.571	0.08
Average Adj. R ²		0.43		0.37	
Average n		1,073		627	

* Bold Text indicates significance at the 0.05 level or better, two-tailed.

Sample period encompasses the 1983 to 2001 time horizon.

p-values are based on the time-series standard errors of the coefficient estimates, where the standard errors are corrected for autocorrelation using the Newey-West procedure. p-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise.

PIN = probability of an information based trade;

MB = market-to-book ratio, is defined as the market value of equity (Compustat #199 * Compustat #25) divided by the book value of equity (Compustat #60);

LEV (leverage) = total debt (Compustat #9 + Compustat #34) divided by total assets (Compustat #6);

MKT CAP (market value of equity) = (Compustat #199 * Compustat #25);

NI = cumulative net income before extraordinary items (Compustat #18) divided by beginning of aggregation period market value of equity, where the aggregation period is either one, two or three years;

RET = buy and hold return over the aggregation period where the aggregation period is either one, two, or three years. Panel C presents the result of Fama-MacBeth regressions estimated annually over the 1983 to 2001 and 1992 to 2001 time horizons;

DIVYLD = common dividends plus share repurchases (Compustat #115 less the change in preferred stock) divided by market value of equity;

%OPTIONS = value of options-based compensation divided by total compensation. We collect compensation data from ExecuComp thereby

restricting the *%OPTIONS* analysis to the 1992 to 2001 time period; and

SIZE = natural log of market value of equity (Compustat #199 * Compustat #25).

TABLE 1
Sample and Descriptive Statistics

Panel A: Descriptive Statistics 1983–2001 (n = 20,389)

	Mean	Median	Std.	Q1	Q3
PIN	0.195	0.182	0.077	0.140	0.235
MB	2.243	1.594	2.259	1.086	2.506
LEV	0.267	0.260	0.179	0.126	0.380
MKTCAP	2888	529	9463	102	2065
NI _{one-year}	0.040	0.068	0.165	0.026	0.106
NI _{two-year}	0.114	0.143	0.265	0.056	0.226
NI _{three-year}	0.210	0.226	0.385	0.089	0.366
RET _{one-year}	0.149	0.105	0.432	-0.113	0.344
RET _{two-year}	0.331	0.235	0.700	-0.111	0.619
RET _{three-year}	0.536	0.363	0.978	-0.074	0.898

Panel B: Correlations Pearson (top)/Spearman (bottom)^a

	PIN	MB	LEV	MKTCAP	NI _{one-year}	NI _{two-year}	NI _{three-year}	RET _{one-year}	RET _{two-year}	RET _{three-year}
PIN										
MB	-0.291									
LEV	-0.085	-0.047								
MKTCAP	-0.700	0.433	0.000							
NI _{one-year}	0.005	-0.052	-0.090	0.138						
NI _{two-year}	-0.044	-0.009	-0.104	0.188	0.792					
NI _{three-year}	-0.072	-0.001	-0.107	0.214	0.674	0.852				
RET _{one-year}	0.003	0.309	-0.089	0.202	0.443	0.286	0.216			
RET _{two-year}	-0.051	0.377	-0.119	0.268	0.444	0.553	0.416	0.659		
RET _{three-year}	-0.099	0.400	-0.134	0.311	0.427	0.548	0.614	0.779	0.753	

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words, the effect of conservatism on information asymmetry is proportional to the pre-existing information asymmetry.

IV. SAMPLE AND DESCRIPTIVE STATISTICS

We restrict our sample to the NYSE and AMEX stocks with December fiscal year-ends over the 1983–2001 time period. The PIN scores are available annually for the NYSE and AMEX sample. As discussed in Easley et al. (2004), the market structures of the NYSE and AMEX rely on specialists and so most closely resemble the structural model used to estimate the PIN score. We require firms to have December fiscal year-ends because the PIN scores are estimated using calendar year trade data over the 1983 to 2001 period.¹³ The PIN score used in our cross-sectional models is the PIN score estimated over calendar year t . We require firms to have the PIN score and sufficient CRSP and Compustat data to be included in our empirical tests.

Table 1 displays the descriptive statistics and the variables' correlation matrix for our sample of 20,389 firm year observations for 1983 to 2001. Panel A reports the sample descriptive statistics.¹⁴ The mean (median) *PIN* is 0.195 (0.182) consistent with the mean *PIN* of 0.208 reported in Easley et al. (2004). The average sample firm has a market-to-book ratio (*MB*; (Compustat #25 * Compustat #199)/Compustat #60) of 2.243 and leverage (*LEV*; (Compustat #9 + Compustat #34)/Compustat #6) of 0.267. In general, our sample is comprised of relatively large firms: the sample mean (median) market value of equity is \$2,888 million (\$529 million). Finally, both the mean and median values of all the scaled net income before extraordinary items (*NI*) and buy and hold return (*RET*) variables are positive indicating that, in general, our sample firms are profitable and have experienced positive returns.

Panel B of Table 1 reports the pairwise correlations among the variables, where the upper-right-hand portion displays the Pearson product-moment correlations, and the lower-left-hand portion displays the Spearman rank-order correlations. To facilitate discussion, we focus on the Pearson correlations, but note that the Spearman rank order correlations are generally consistent with the Pearson results. The *PIN* score is significantly negatively correlated with *MB*, *LEV*, *MKTCAP*, all the scaled net income variables and the three-year buy and hold return. The negative relation between *MB* and *PIN* is likely driven by the fact that our sample is comprised of relatively large firms. The negative correlations between *PIN* and *LEV* (−0.060) and *PIN* and *MKTCAP* (−0.301) are consistent with at least a portion of the information asymmetry captured by the *PIN* being due to variation in firms' investment opportunity sets (Smith and Watts 1992). All of the scaled net income and return measures are positively correlated with each other indicating that accounting earnings capture at least a portion of the information reflected in returns.

An examination of the empirical relation between *PIN* and corporate policies provides useful information for this study. First, to the extent that *PIN* is associated with corporate policies and measures of growth options in the directions predicted by extant studies, we can gain more confidence that *PIN* is measuring information asymmetries. Second, understanding the relation between information asymmetries and policies that involve contracts (debt and compensation policies) that generate conservatism can help identify the marginal effect of information asymmetry on conservatism.

¹³ Our results are not sensitive to using only December fiscal year-end firms.

¹⁴ To reduce the influence of extreme observations we winsorize, all the *NI* and *RET* variables at the 1st and 99th percentile values.

If changes in information asymmetry lead conservatism (H2c), then we expect the coefficient of $\Delta PIN_{t-1} * RET_t * DR_t$ (β_7) to be positive and significant. If changes in information asymmetry are contemporaneous with changes in conservatism, then we expect the coefficient of $\Delta PIN_t * RET_t * DR_t$ (β_7) to be positive and significant. Finally, if changes in conservatism lead changes in information asymmetry (H3), then we expect the coefficient of $\Delta PIN_{t+1} * RET_t * DR_t$ (β_7) to be positive and significant.

To test whether changes in information asymmetry lead the extent to which: (a) gains are reflected in current financial statements (H2a); and (b) losses are reflected in current financial statements (H2b), we use the following specification for the positive and negative return samples separately:

$$NI_t = \alpha + \beta_1 RET_t + \beta_2 \Delta PIN_{t+x} + \beta_3 \Delta PIN_{t+x} * RET_t + \beta_4 PIN_{t-1+x} + \beta_5 PIN_{t-1+x} * RET_t + \varepsilon_t \quad (5)$$

where firm subscripts are omitted. All variables are as previously defined.

Hypothesis 2a predicts the coefficient of $\Delta PIN_{t-1} * RET_t$ (β_3) to be negative and significant for the positive return sample estimations, while H2b predicts coefficient β_3 to be positive and significant for the negative return sample estimations.

It is important to realize that the effect of a change in information asymmetry on conservatism can be both first-order and timely. If a firm begins investing significantly in growth options or acquires another firm that has significant growth options, then the evidence in finance on corporate policies suggests the *increase in information asymmetry* would be significant. The firm's stock return would depend much more on changes in those growth options' value and the extent to which both good and bad news is reflected in earnings would change in a timely fashion. As soon as the investment is made, less relative good news in stock returns would be related to current cash flows and β_3 *would tend to be negative for the positive returns sample*. The increase in information asymmetry would cause less relative bad news to be related to current cash flows, but more write-downs of firm-specific assets whose value is dependent on the projects' success. This would tend to produce a *positive β_3 for the negative returns sample*.

If a firm divests itself of a growth subsidiary or its investments begin to mature and the ratio of current cash flows to expected future cash flows increase, then we expect *information asymmetry to decline* and current earnings to reflect relatively more of any good news on the firm in a timely fashion. The estimate of the β_3 *coefficient in the positive returns regression should be negative*. Likewise, with less growth options and less information asymmetry we expect fewer firm-specific assets and fewer write-downs with bad news, producing a *positive β_3 for the negative returns sample*.¹²

Also note that we have only briefly discussed the effect of management manipulation of earnings on our predicted results. We argue that the ability to manipulate varies positively with the existence of growth options and that conservatism is an equilibrium mechanism for reducing that manipulation. For the empirical work we assume that in equilibrium the information asymmetry remaining after the effects of conservatism varies directly with the information asymmetry associated with information before conservatism. In other

¹² Note that preceding analysis does not allow for changes in the capitalized value of rents (goodwill). In doing so we rely on the arguments and evidence in Roychowdhury and Watts (2007) that neither increases nor decreases in growth options are recorded, at least in our sample period, which is prior to the SFAS No. 142 changes in the standards for goodwill impairment.

We estimate Equation (3) to test H1c:

$$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \beta_4 PIN + \beta_5 PIN * DR + \beta_6 PIN * RET + \beta_7 PIN * RET * DR + \varepsilon \quad (3)$$

where firm and time subscripts are omitted. All variables are as previously defined.¹¹ Hypothesis 1c predicts the coefficient of $PIN * RET * DR$ (β_7) will be positive and significant, indicating that as information asymmetries between managers and outside investors increase, the asymmetric recognition of gains and losses in current financial statements increases.

To conduct our main tests of our second and third hypotheses we incorporate the change in the PIN score at different time intervals $t-1$, t , and $t+1$ into Equation (1) as follows:

$$\begin{aligned} NI_t = & \alpha + \beta_1 DR_t + \beta_2 RET_t + \beta_3 RET_t * DR_t + \beta_4 \Delta PIN_{t+x} + \beta_5 \Delta PIN_{t+x} * DR_t \\ & + \beta_6 \Delta PIN_{t+x} * RET_t + \beta_7 \Delta PIN_{t+x} * RET_t * DR_t + \beta_8 PIN_{t-1+x} \\ & + \beta_9 PIN_{t-1+x} * DR_t + \beta_{10} PIN_{t-1+x} * RET_t \\ & + \beta_{11} PIN_{t-1+x} * RET_t * DR_t + \varepsilon_t \end{aligned} \quad (4)$$

where firm subscripts are omitted. ΔPIN_{t+x} is equal to the change in the PIN score over period $t+x$, where x is equal to one of the following: -1 , 0 , 1 . All other variables are as previously defined.

In Equation (4) we vary the timing of the change in the PIN score to test whether (1) changes in the level of information asymmetries between managers and outside investors lead the asymmetric recognition of gains and losses in current financial statements (H2c); and (2) conservatism leads changes in the level of information asymmetries between managers and outside investors (H3).

The structure of the cross-sectional estimation of the Basu coefficient makes the estimation of changes in conservatism difficult: the differential reflection of good news (positive returns) and bad news (negative returns) in income is a function of the sign of the news in the period. That dichotomy does not translate well into changes in news and conservatism. Instead of estimating the relation between changes in conservatism (the Basu coefficient) and changes in information asymmetry (PIN) directly we estimate the time-series properties of annual PIN scores and use those properties to specify Equation (4).

The correlations of the overtime changes in PIN are -0.43 , 0.00 , and -0.02 for lags 1, 2, and 3, respectively. The correlations suggest that PIN changes consist of the shock to the current period PIN and a fraction of the shock to the preceding period's PIN. Consequently, we split the annual PIN score into two parts: the PIN level for the previous year and the change in PIN for the year. Based on the correlation structure, we expect the previous year's PIN level to pick up the reversal of last year's shock in the current year. That leaves the PIN change (ΔPIN) to pick up the current year's shock to PIN and measure the timing of changes in information asymmetry.

¹¹ We present the results using the year t PIN score in each specification, allowing us to use the same sample of firms for one-, two-, and three-year specification. The year t PIN is estimated using the trading that took place during fiscal year t . In untabled analysis we match the PIN lag with the estimation period, i.e., the two- (three-) year estimation with the average PIN over the two- (three-) year period. The signs and statistical significance of the coefficients in this analysis are similar to those reported in the tables.

measure information asymmetry between informed and uninformed investors in equity markets, where management is part of the informed group. As such, it fits our purpose of examining the effects of information asymmetry between managers and equity investors.⁹

PIN is a function of the abnormal order flow. The underlying assumption is that public information is directly incorporated in prices because the market maker would move prices to the appropriate level at the time of the information and there would not be any trading activity. Private information generates excess buying or excess selling pressure (abnormal order flow) depending on the nature of the information. On a day during which bad private information arrives, there are more sell orders than buy orders, and conversely on good private information day, there are more buy orders than sell orders.

The general intuition behind the PIN measure is that firms that exhibit large deviations from their normal level of order flow most likely have higher levels of trading based on private information, which results in deviations from the normal level of order flow. The PIN score is derived from a structural market microstructure model, using individual firm trade data and maximum likelihood estimation to estimate the structural market microstructure model. Specifically, the PIN score is the arrival rate of information-based orders divided by the arrival rate of all orders, where the arrival rates are parameters from the structural model. It then follows that higher PIN scores represent higher probabilities of information based trading, i.e., greater information asymmetries.

A series of papers have used structural models of trading to estimate the PIN score. These studies investigate issues spanning the pricing of information risk (Easley et al. 2002, 2004; Botosan and Plumlee 2004), variation in post-earnings announcement drift (Vega 2006), the effect of disclosure on information asymmetry (Easley et al. 1998; Brown et al. 2004), the sensitivity of investment to stock prices (Chen et al. 2007), and various issues related to market microstructure (Easley, Kiefer, and O'Hara 1996, 1997a, 1997b; Easley, Kiefer, O'Hara, and Paperman 1996). These studies find evidence consistent with the PIN capturing information asymmetries. We use firm-year measures of PIN generated by Easley et al. (2004) for the period 1983–2001 as our primary measure of information asymmetries.¹⁰

Primary Empirical Specification

To test H1a and H1b we estimate Equation (2) for the positive and negative return samples separately:

$$NI = \alpha + \beta_1 RET + \beta_2 PIN + \beta_3 PIN * RET + \epsilon \quad (2)$$

where firm and time subscripts are omitted. *PIN* is the probability of an information-based trade measured using the trading data over the fiscal year. All other variables are as previously defined.

Hypothesis 1a predicts the coefficient of *PIN * RET* (β_3) will be negative and significant for the positive return sample estimations while H1b predicts coefficient β_3 will be positive and significant for the negative return sample estimations.

⁹ Information asymmetry in debt contracting is between different parties. For example, in debt contracting the important information asymmetry is between managers and the banks that lend privately to firms. It is those contracts that are the most binding and yield most of the debt covenant violations (Sweeney 1994, 290). The banks making the private loans are likely to be more informed than the uninformed equity investor. Similarly, in compensation contracting the important asymmetry would seem to be between the manager and the board of directors.

¹⁰ We thank Soeren Hvidkjaer for making the PIN scores available on his website: <http://www.smith.umd.edu/faculty/hvidkjaer>.

is a measure of the asymmetric verifiability of gains and losses that generates the understatement of net assets. The larger the asymmetric verifiability, the larger is the understatement of net assets. The MB ratio includes both the understatement of the firm's net assets and unverifiable growth options or rents (see Watts 2003a; Roychowdhury and Watts 2007). Given conservatism is the understatement of net assets, both the Basu coefficient and MB measure conservatism with error.

Annual horizon estimates of the Basu coefficient are affected by firms' failure to record asset write-downs because previous asset value increases were not recorded due to conservatism (the "buffer" problem). This effect causes the annual Basu coefficient to understate the degree of conservatism (Pae et al. 2005). Roychowdhury and Watts (2007) provide evidence that the buffer problem is significantly reduced by estimating the Basu coefficient using three-year estimation intervals.⁸

Growth options are included in market value, but book value incorporates those growth options only when acquired. This causes MB to measure the understatement of assets with error (see Holthausen and Watts 2001, 32–36). Roychowdhury and Watts (2007) argue (and present indirect evidence) that MB measures conservatism with more error than the Basu coefficient when the Basu coefficient is estimated using longer estimation intervals. Their results suggest that in practice neither increases nor decreases in growth options are recorded, consistent with accounting measuring net assets rather than the value of equity. As a result, the Basu coefficient tends not to incorporate changes in growth options and is not significantly affected by the buffer problem when estimated for longer horizons. Consequently, we use the Basu coefficient to measure conservatism in our hypotheses tests.

Basu (1997) develops a measure of the differential verifiability required for the recognition of accounting gains versus losses, conservatism. Specifically, Basu (1997) estimates the following model:

$$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \epsilon \quad (1)$$

where firm and time subscripts are omitted. *NI* is net income before extraordinary items (Compustat #18) reported in period *t* divided by beginning of fiscal year market value of equity (Compustat #25 * Compustat * #199), *RET* is the buy and hold return over the fiscal year, and *DR* is equal to 1 if *RET* is negative, and 0 otherwise.

The Basu model uses returns, *RET*, as a proxy for news, where positive returns proxy for good news and negative returns proxy for bad news. By allowing differential sensitivity of net income to negative returns, *RET* * *DR*, the Basu model captures the differential verifiability required for the recognition of gains versus losses, conservatism. Following Roychowdhury and Watts (2007), in our empirical tests we use various horizons (one year, two years, and three years) to measure conservatism and the influence of information asymmetries on conservatism.

Information Asymmetry Measure (PIN Score)

To test our hypotheses we require a measure of information asymmetry between managers and outside equity investors. The PIN was designed by Easley and O'Hara (1992) to

⁸ Roychowdhury and Watts (2007) find that over longer horizons the association between Basu's measure of conservatism and MB flips from negative to positive. While the negative association between the Basu coefficient and MB has been used as evidence against the Basu coefficient capturing conservatism, Roychowdhury and Watts (2007) demonstrate that this negative association is due to relatively greater dependence of short period Basu estimates on the composition of MB at the beginning of the estimation period.

H1c: The greater the information asymmetry between insiders and uninformed equity investors, the larger the asymmetric recognition of gains and losses in current financial statements.

The big bath, earnings management, hypothesis is an alternative hypothesis for both H1b and H1c. In the big bath hypothesis, managers take advantage of losses and large stock price drops to write net assets down below their market value and use the consequent reserve to smooth future earnings. It is possible that losses, large stock price drops, and big baths are associated with increased information asymmetry between insiders and outside investors. However, Watts (2003b, 296–297) argues that the big bath hypothesis is not consistent with the overall pattern of the evidence on conservatism and so cannot by itself explain that evidence. Further, it is not obvious that a big bath would generate H1a.

Because we argue that information asymmetry generates conservatism, we expect information asymmetry changes to lead conservatism. This provides our second hypothesis:

H2: Changes in information asymmetry lead conservatism.

This hypothesis is also tested in three parts:

H2a: Changes in the information asymmetry between insiders and uninformed equity investors lead the extent to which gains are reflected in current financial statements;

H2b: Changes in the information asymmetry between insiders and uninformed equity investors lead the extent to which losses are reflected in current financial statements; and

H2c: Changes in the information asymmetry between insiders and uninformed equity investors lead the asymmetric recognition of gains and losses in current financial statements.

The FASB contends that conservatism generates information asymmetry by not informing investors of the future cash flows from growth options (FASB 2005). Their argument implies that changes in conservatism lead changes in information asymmetry between informed and uninformed equity investors. This provides Hypothesis 3:

H3: Conservatism leads changes in information asymmetry.

This FASB argument does not specify what is expected with respect to gains and losses.

Hypotheses 2 and 3 are not alternatives of each other. The null of H2 is that changes in information asymmetry follow or are contemporaneous with greater conservatism. The null of H3 is that greater conservatism follows or is contemporaneous with changes in information asymmetry.

III. RESEARCH DESIGN

Conservatism Measure

Conservatism can be measured by the Basu coefficient and/or the ratio of the market value of equity to the book value of equity (market-to-book [MB]). The Basu coefficient

serves as a benchmark for *competing, multiple* softer information sources. Those sources include both management and sources independent of management. Investors can compare the different sources' predictions to the hard numbers that are eventually realized. This enables them to evaluate the competing sources' reliability.

The ability to use the benchmark to evaluate competing sources allows those information sources to build reputations and credibility if they, in turn, have governance arrangements such as deferred compensation plans. The reputations bond the sources against issuing intentionally misleading information. Outside information suppliers are not limited to financial reports and disclosures by management; they can also obtain information from multiple other sources such as suppliers, customers, etc. Published verifiable financial accounts provide a mechanism for alternative sources of information to exist and to reduce information asymmetry between equity investors. Further, the competition between sources disciplines both the sources' themselves and the managers of the firms on which they provide information.

Hypotheses

By their nature, growth options (future positive NPV projects) are unverifiable and so generate information asymmetry between managers and outside investors (Smith and Watts 1992). This information asymmetry provides managers with the opportunity to manipulate accounting numbers to try to mislead outside investors. In particular, managers will tend to overstate gains and understate losses. Conservative accounting provides assurance to outside investors that gains will not be overstated and losses understated. Increases in growth options are not recorded because they are unverifiable, so, as information asymmetry increases, fewer gains that are reflected in stock return are reflected in current earnings. Similarly, as total information asymmetry increases, from the outside investors' and auditor's perspective, it is likely that more and more projects have a likelihood of loss. The increase in the likelihood of loss follows from the increase in the variance of anticipated returns from the projects resulting from the information asymmetry.⁷ The result is that we expect firms with more information asymmetry between insiders and uninformed outside investors to have more losses reflected in current earnings. The net result is that conservatism in the form of the differential recognition of gains and losses (the Basu coefficient) will increase as information asymmetry between insiders and uninformed investors increases.

The above reasoning provides us with our first hypothesis:

H1: The more the information asymmetry between insiders and uninformed equity investors, the larger the conservatism in the firm's financial statements.

We test this hypothesis in three parts:

H1a: The greater the information asymmetry between insiders and uninformed equity investors, the less gains are reflected in current financial statements;

H1b: The greater the information asymmetry between insiders and uninformed equity investors, the more losses are reflected in current financial statements; and

⁷ As growth options increase, so will the variance of anticipated profits due to the fact that a greater proportion of these profits will be tied to the growth options, which may or may not prove successful. Consistent with this statement, Khan and Watts (2007) find an increase in stock return volatility leads to an increase in conservatism.

Increases in growth options also limit the ability of accounting-based debt covenants to control the agency problems of debt. Again, including unverifiable future cash flows in accounting earnings does not appear to reduce information asymmetry and solve the problem. Instead as growth options increase, the use of debt declines presumably because of the decreasing ability to use reported numbers to reduce the agency costs of debt. Eventually, the firm is fully equity-financed (Smith and Watts 1992).

Two potential alternative mechanisms for reducing information asymmetry with the provision of unverifiable information are the use of reputation to bond the manager supplying the information and signaling. The lack of use of unverifiable information in contracting suggests those mechanisms do not work. One possible reason is that managers have a relatively limited tenure to build a reputation and, even if they can build a reputation, there is still an end-game problem (retirement). Compensation contracts partially address the end-game problem by using conservative accounting and deferring payments of bonuses (either cash or stock) to future periods. The contracts also provide for forfeiture of bonuses not yet paid “if and when the compensation committee finds that the manager committed ‘any act of omission or commission prejudicial or detrimental to the interests’ of the firm” (Smith and Watts 1982, 146).⁵

Conservatism and the Information Environment

If accounting cannot solve the problem of reducing information asymmetry by providing unverifiable information, then how does conservative accounting reduce information asymmetry between equity investors? Two potential mechanisms suggest themselves. First, conservative accounting could well provide the best possible non-stock price “hard” summary information on current performance for uninformed investors. Second, that hard information provides a benchmark that makes it possible for alternative “soft” sources to generate credible information on unverifiable gains.

Hard Information on Current Performance

Given managers’ incentives to overstate unverifiable gains, verifiable gains are likely to be the only “hard” information accounting can supply on gains. On the other hand, generally managers will tend to be reluctant to report less verifiable losses. As we argued earlier, a lower verification standard for losses is likely to elicit information managers are reluctant to provide, suggesting, on average, that loss information is reliable. The net result from conservative accounting then could be the provision of more information than would be provided by an accounting regime that applies equally strong verification standards to both gains and losses.⁶

Benchmark for “Soft” Information on Current and Future Performance

Lambert (1996), Ball (2001), and Watts (2006) hypothesize that financial statements discipline other sources of information. Income statement reporting of verifiable current and future cash flow realizations limits management’s ability to manipulate financial statements and overstate earnings. The resultant balance sheet net assets are also verifiable and limit managements’ ability to manipulate and overstate the resources available to the firm. The cash flow realizations and the accumulation of *verifiable* net assets provide credible evidence on the outcome of previous investments and growth options. That evidence then

⁵ The internal quote is from Gulf Oil Corporation’s Incentive Compensation Plan, May 1, 1973, p. 12.

⁶ It should be noted that conservative accounting can fulfill a similar disciplining role within the firm where there is information asymmetry between employees and their supervisors.

and the resources he has to control depend on the firm's stock return. As in contracting, in this scenario the manager can use his information advantage to try to shift wealth from investors to himself (Watts 2003a, 215), increasing reported gains and reducing reported losses in attempt to increase stock returns.

Financial reporting manipulation uses up resources (including management time) and diverts managers' efforts from increasing the total wealth available to all the parties to the firm (Jensen and Meckling 1976; Watts and Zimmerman 1986). As a result, the manipulation generates deadweight losses and reduces the firm's cash flow and its stock price. The information asymmetry generates agency costs. This effect also provides incentives for all parties to the firm to find ways (e.g., combinations of accounting methods, governance mechanisms, etc.) to reduce management's private information relative to public information.

Managers will tend to engage in such accounting manipulation even if, on average, the stock and managerial labor markets are efficient and stock prices adjust for the manipulation (see Watts and Zimmerman 1986, 208). Otherwise, the manager suffers the penalties (reduced compensation) without achieving the gains. Regardless of whether the manager benefits on net, the manipulations generate agency costs just as in contracting. And, just as in contracting, the manager and the other parties to the firm have incentives to reduce those deadweight losses by finding mechanisms to make the information supplied to investors credible.

Verifiability and Financial Reporting Information

Investors and potential investors in the firm's securities demand information, not only on the firm's past financial performance and its net assets, but also on its expected future performance. (e.g., its future cash flows). However, there are limits to the extent to which management-provided information can be credible. As information becomes less verifiable, it becomes easier for the manager to manipulate and less credible, making it less useful to investors.

Given economic Darwinism, we expect the evolution of financial reporting practice and the contracting use of financial reporting numbers to provide evidence on the relation between verifiability and information. The evolution of Anglo-American practice suggests that only some future cash flows (e.g., accounts receivable) are sufficiently verifiable to limit manipulation and, on net, provide information to investors (Watts 2006). The financial reporting numbers used in debt and compensation contracts reflect a similar trade-off with only relatively limited inclusion of future cash flows (Leftwich 1983; Beatty et al. 2006). The lack of contracting use is important because the contract parties are relatively well informed, well motivated, and internalize the costs and benefits of more, versus less, verifiable information.

A sense of verifiability's effect on reported information's usefulness can be obtained by investigating the extent to which the use of debt and compensation contracts varies with information asymmetry between managers and other parties. Information asymmetry varies with the proportion of a firm's value that is represented by growth options—positive net present value (NPV) projects that are a function of future cash flows (Smith and Watts 1992, 275–276). The evidence from the compensation market suggests the sum of shareholders' and managers' wealth is decreased if unverifiable future cash flows are included in accounting earnings and used to evaluate performance. As growth options increase, the use of reported earnings-based compensation contracts declines (Smith and Watts 1992, Table 1, 270) and the use of stock price-based compensation increases (Smith and Watts 1992, Table 1, 270; Gaver and Gaver 1993, Table 8, 152).

asymmetric verifiability following that increase (decrease) generates more (less) conservatism. Overall, our results are consistent with our predictions that the greater the information asymmetry, the more conservative are the firm's financial statements and that information asymmetry changes lead conservatism.

In Section II we expand on our arguments as to why information asymmetry between insiders (managers) and outside equity investors generates conservatism in financial statements. Section II also develops our testable hypotheses. In addition, in this section we argue that conservative financial reports are likely to generate a more informed capital market than financial reports that include unverifiable information. That implication is, however, not tested in this paper. Section III presents the methods used to test our hypotheses and discusses our measure of information asymmetry, the PIN score. The data is described in Section IV. Section V reports the test results and Section VI discusses the result of various robustness tests. Our summary, conclusions, and standard-setting implications are given in Section VII.

II. CONSERVATISM'S INFORMATION ROLE

In this section, we first explain why information asymmetry between inside and outside investors reduces the firm's stock price. One potential reason is that it increases the required rate of return on the stock. A second reason is that the information asymmetry generates agency costs that reduce the firm's expected cash flows. We next explain how these effects lead to conservatism's information role. Conservatism reduces information asymmetry by restricting the manager's ability to manipulate financial reporting and by providing an environment that allows other sources of information to flourish. Finally, we develop and specify our hypotheses.

Information Asymmetry and Stock Prices

Information asymmetry between security traders affects security prices. This hypothesis goes back at least to Jack Treynor (Bagehot 1971) who suggests information-based trading affects the spread between bid and ask prices offered by market specialists. The greater the relative private information, the larger the bid-ask spread and the lower the returns to investors without private information, in particular non-insiders, and the higher the equilibrium required returns on the stock (Amihud and Mendelson 1986). More recently, Easley and O'Hara (1992) develop a model of abnormal order flow that provides an estimate of the relative amount of private-information-based trading for a given stock: the probability of an information-based trade (PIN). In the model, public information is incorporated in prices without trading while private information is reflected in excess buying or selling pressure. Easley and O'Hara (2004) develop a theoretical model that predicts that securities with greater private information relative to public information have a higher equilibrium required rate of return. Easley et al. (2002) provide empirical evidence consistent with that prediction.

The equilibrium return effect of informed trading by management and other insiders provides the parties to the firm with incentives to institute governance mechanisms that increase public information and so reduce private information and increase stock price. However, the increased equilibrium return is not the only potential effect of information asymmetry on stock price—there is an effect on the level of the firm's cash flow. Management has incentives to use their private information to manipulate earnings and other financial information and transfer wealth to themselves, even in the absence of financial-reporting-based debt and compensation contracts. The manager's likelihood of keeping his position, his compensation, his receipts from selling shares acquired by exercising options,

The information asymmetry changes test relies on relatively prompt changes in conservatism in reaction to changes in information asymmetry. There are reasons to believe that there will be a significant number of cases where conservatism responds promptly to information asymmetry changes (in the same year or the next). Changes in information asymmetry and changes in conservatism occur because of *changes* in the investment opportunity set and some of those changes are often promptly reflected in the accounting. Examples of those changes include spin-offs, acquisitions, maturation of a firm's investments, etc. We expand on the reasons we expect prompt conservatism changes in Section III.

We test our predictions using a sample of 20,389 firm-year observations over the 1983 to 2001 time period. Conservatism is measured using the Basu (1997) model that regresses earnings on returns and allows the return coefficient to vary with the sign of the return. The estimated incremental coefficient on negative returns (incremental to the coefficient on positive returns), the Basu coefficient, is our conservatism measure. The information asymmetry measures are the PIN score (Easley et al. 2002) and the bid-ask spread. Both are larger, the greater the information asymmetry between managers and outside uninformed investors.³

Using cross-sectional Fama and MacBeth (1973) regressions we find that the Basu coefficient is larger, the larger the information asymmetry measure. In other words, firms with higher information asymmetry between insiders and outside equity investors report more conservative earnings. These results are robust (1) to controls for other factors associated with conservatism, specifically the market-to-book ratio, leverage, regulation, and litigation, and (2) to estimation of the Basu coefficient over multi-year as well as one-year intervals. The controls for the factors associated with conservatism generate a strong test of the first prediction since those factors are all theoretically and empirically associated with the existence of growth options and information asymmetry.

To test whether information asymmetry changes lead or lag conservatism we examine the association between annual conservatism measures and PIN score changes in the preceding year, the contemporaneous year, and the following year. We find that PIN score changes in the preceding and contemporaneous year are both positively associated with the annual conservatism measure. This means changes in information asymmetry both lead and are contemporaneous with conservatism.⁴ Consistent with this interpretation, we find that information asymmetry changes in the preceding year are negatively associated with the extent to which good news is reflected in annual earnings. This result implies the increased information asymmetry makes good news less verifiable in the future. These results are also robust to the use of bid-ask spread as the measure of information asymmetry.

The combined results of the hypotheses tests are consistent with the implementation of existing accounting and auditing standards reflecting conservatism that reduces information asymmetries. When relatively more of a firm's gains are unverifiable, the application of the asymmetric verifiability standards generates more conservatism. When the information asymmetry between equity investors in a firm increases (decreases), the application of the

³ The PIN score is the probability of an information-based trade derived from a structural market microstructure model (see Easley et al. 2002, 2004). Numerous prior studies (discussed in Section IV) use the PIN score to capture difference in information asymmetries between informed and uninformed investors. In general the results of these studies are consistent with PIN score differences reflecting differences in asymmetric information across firms.

⁴ Subsequent research, Khan and Watts (2007), using event study methodology and a firm-year conservatism measure that does not require negative returns, finds conservatism peaks in the year following an increase in information asymmetry as measured by bid-ask spread.

statements affect the manager's welfare and therefore his incentives in other ways as well. The firm's stock price, the manager's likelihood of keeping his position, his informally determined compensation, the value of his stock options and the resources under his control (firm size) are all affected by the firm's financial statements even in the absence of formal accounting-based contracts. The manager's asymmetric loss function gives him incentives to use his private information to attempt to transfer wealth from investors to himself by overstating financial performance and consequently stock prices during his tenure at the firm. The fact that managers and their firms are net sellers of their stock during the managers' tenure reinforces their incentives to overstate.¹

Markets recognize managers have incentives to overstate. Outside, uninformed buyers recognizing the information asymmetry and the adverse selection problem, reduce their bid prices for shares. Further, the managers' time spent on manipulation and overstatement deflects their efforts from increasing firm value, generating agency costs, and reducing firm value even more. We argue conservative financial reporting is a governance mechanism that reduces the managers' ability to manipulate and overstate financial performance and increases the firm's cash flows and value.

Ceteris paribus, the preceding arguments imply firms with greater conservatism have lower information asymmetry between managers and outside investors. However, we expect information asymmetry to vary with the firm's investment opportunity set. By their nature, growth options (future positive net present value projects) are unverifiable so that information asymmetry between managers and outside investors is expected to be larger, the greater those growth options (Smith and Watts 1992). Like Smith and Watts (1992) and Gaver and Gaver (1993), we assume that the investment opportunity set is exogenous. If conservatism reduces the information asymmetry generated by growth options proportionately, then we expect conservatism to vary positively with observed information asymmetry.

Our arguments imply that information asymmetry engenders conservatism. Consequently, in this paper we test two predictions:

1. the larger the information asymmetry between inside and outside investors, the more conservative are the firm's financial statements; and
2. information asymmetry changes lead conservatism changes.

Since other conservatism explanations (i.e., contracting and litigation) imply information asymmetry generates conservatism, our tests control for those explanations. Also, since some FASB statements imply conservatism engenders information asymmetry rather than *vice versa*, we also test that implication directly.²

¹ Using a large sample of insider trading over the period 1988–2000, we find that in ten of the 13 years insiders were net sellers and on average insiders had net sales of over \$4 billion a year.

² The FASB has consistently opposed conservatism, for example see SFAC No. 2 (FASB 1980, para. 93) which states that the “notion became deeply ingrained and is still in evidence despite efforts over the past 40 years to change it.” In a handout for both the FASB's July 27, 2005 and the IASB's July 20, 2005 meetings on the conceptual framework (FASB 2005, 24), the FASB responds to questions on “What is the role of conservatism? Does it conflict with neutrality? If not, why not? Why keep it?” with:

Financial information needs to be neutral—free from bias intended to influence a decision or outcome. To that end, the common conceptual framework should not include conservatism or prudence among the desirable qualitative characteristics of accounting information. However, the framework should note the continuing need to be careful in the face of uncertainty.

There certainly appears to be a view that the understatement of net assets and cumulative profits generated by conservatism will cause users of financial statements, including investors, to make incorrect inferences presumably because of uncertainty about the bias. The FASB response implies conservatism increases information asymmetry and so increases in conservatism lead to increases in information asymmetry.

The Information Role of Conservatism

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ABSTRACT: In this paper we argue that information asymmetry between firm insiders and outside *equity* investors generates conservatism in financial statements. Conservatism reduces the manager's incentives and ability to manipulate accounting numbers and so reduces information asymmetry and the deadweight losses that information asymmetry generates. This increases firm and equity values.

Our empirical tests are consistent with our proposition that information asymmetry is significantly positively related to conservatism after controlling for other demands for conservatism. Further, our tests are more consistent with our prediction that changes in information asymmetry between equity investors lead to changes in conservatism than the FASB's proposition that conservatism produces information asymmetry among equity investors. An important implication is that, if the FASB was successful in meeting its stated goal of eliminating conservatism, then it would *increase* information asymmetry between investors, not *reduce* it. This outcome is inconsistent with the objectives of the Securities Acts.

Keywords: *conservatism; accounting standards; earnings management; corporate governance.*

I. INTRODUCTION

Conservatism is the differential verifiability required for the recognition of accounting gains versus losses that generates an understatement of net assets (Basu 1997; Holthausen and Watts 2001, 37). The existing empirical evidence is consistent with listed firms' financial statements being conservative in the U.S. (e.g., Basu 1997; Watts 2003b) and in other Anglo-American law countries (e.g., Ball et al. 2000). This conservatism is attributed to the use of financial statements in debt and/or compensation contracts, litigation, regulatory and political processes, and taxes (Watts 2003a, 2003b). In the contracting explanations, conservatism occurs because it reduces agency costs associated with (1) asymmetries in information and loss functions among the contracting parties; and (2) an inability to verify the more informed parties' private information.

In this paper we argue that agency costs generated by the manager's private information and asymmetric loss function are not limited to debt and compensation contracts. Financial

We thank Soeren Hvidkjaer and Joel Hasbrouck for some of the data used in the study and gratefully acknowledge comments by Anwer Ahmed, Holly Skaife, Paul Asquith, Robert Holthausen, Mozaffar Khan, S. P. Kothari, Karthik Ramanna, Sugata Roychowdhury, Florin Vasvari, Joseph Weber, Yong Zhang, Jerry Zimmerman, two anonymous referees, and the workshop participants at the University of Edinburgh, Harvard University, Humboldt University Berlin, Massachusetts Institute of Technology, London Business School, and The Pennsylvania State University.

Editor's note: This paper was accepted by Dan Dhaliwal.

*Submitted October 2006
Accepted September 2007*

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sample, 53 percent report telemarketing expenses as other than professional fundraising fees and, at least, another 4 percent is allocating telemarketing costs to an expense category other than fundraising.

Our results provide strong evidence that nonprofits misreport telemarketing fees, which affects how program and fundraising ratios are reported. The effect on reported ratios of misreporting is substantial. We find that by misreporting telemarketing expenses the nonprofits in our sample could understate the fundraising ratio by as much as 15 percent.

Of the misreporting we detect, most occurs among small nonprofits that have limited accounting sophistication. Our findings suggest that nonprofits that have greater accounting sophistication and those likely to be subjected to greater external monitoring are less likely to be classified as a misreporting firm. We find that the factors associated with the more prevalent activity of misreporting revenue differ from those related to expense classification and allocation. Higher accounting sophistication and more external monitoring appear to play a greater role in moderating revenue misreporting. Only the use of professional outside accountants appears related to proper classification of telemarketing costs as professional fees. We interpret these results as suggesting that misreporting decisions may be driven either by incentives to improve reported results or a lack of familiarity with accounting. Prior research has implicitly or explicitly attributed misreporting to managerial incentives. Our study is the first to specifically consider accounting sophistication as a factor in misreporting.

SOP 98-2 requires nonprofit organizations to allocate costs incurred jointly for fundraising and program activities to several expense categories. However, the occurrence of expense allocation should be related to the joint activity, not systematically associated with organizational characteristics. Allocation of telemarketing costs to an expense category other than fundraising is less often associated with larger organizations and those that have relatively higher levels of debt. This finding implies that allocation may occur more often in small organizations in order to improve reported fundraising ratios, or is more prevalent in organizations that have less accounting sophistication or fewer monitoring mechanisms.

These findings can inform the current debates by state and federal regulators as they search for ways to improve the quality of nonprofit financial reports. In particular, we provide evidence to policy makers that, in addition to regulation and monitoring, educating Form 990 preparers can improve accounting quality.

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report the “telemarketing” fee on line 9b (direct expenses of special events). To address our concerns about possible special events services provided to nonprofit organizations, we create an alternative measure of *REVDIFF* to compare total telemarketing revenues to contributions plus special event revenue from Form 990. Similarly, we measure *FEEDIFF* to compare total telemarketing costs to professional fundraising fees plus direct costs of special events and *FUNDEXPDIFF* to compare total telemarketing costs to total fundraising expense plus direct costs of special events. Our results are robust to these alternative measures of *REVDIFF*, *FEEDIFF*, and *FUNDEXPDIFF*.

Finally, rules governing the allocation of joint costs changed in 1998 with the issuance of SOP 98-2. To determine whether the likelihood of allocating telemarketing expense out of fundraising expense differs as a result of SOP 98-2, we include a dummy variable, *SOP98-2*, in the regression (using “both sources”) that equals 1 if Form 990 is dated after 1998, and 0 otherwise. The coefficient on this variable is not significant and its inclusion does not affect the regression results.

VI. CONCLUSIONS

Prior research has supported a concern by regulators and donors that nonprofits have incentives to understate fundraising costs and may inappropriately allocate these costs to other activities. Additionally, a number of studies provide evidence that donors direct their charitable gifts to nonprofits that report higher program ratios and lower fundraising ratios. With more than 76 percent of the more than \$240 billion in annual contributions to nonprofits in the U.S. coming from individual donors (American Association of Fundraising Counsel [AAFRC] Trust for Philanthropy 2003), misreporting by nonprofits can potentially have a large effect on the distribution of donations among nonprofit organizations.

Our study provides empirical evidence of how frequently fundraising costs are misreported, and examines the methods used and the factors associated with these decisions. This study directly tests the veracity of nonprofits’ reporting practices by comparing federally mandated nonprofit financial reports to disclosures of revenues and costs of telemarketing campaigns filed by telemarketing solicitors in certain states. Additionally, it is the first paper to specifically consider the effect of accounting sophistication on nonprofit reporting practices.

We design our tests to produce conservative estimates of telemarketing revenue and expense by using only the single largest reported telemarketing campaigns conducted each year for a nonprofit by each of its telemarketing solicitors. These estimates of total annual telemarketing revenues and expenses are then compared to the nonprofit’s annual IRS informational filing. Because our design biases against incorrectly labeling a nonprofit a misreporter, we may not have fully detected net reporting, particularly by organizations with contributions raised without the assistance of professional solicitors. This is particularly a concern for the larger organizations in our sample as they are more likely to generate contributions from multiple sources. Thus, we may have underestimated the degree to which misreporting occurs.

Despite our conservative test design, we find that over 74 percent of the organizations in our sample fail to properly report telemarketing expenses. Twenty-seven percent of firm-years contain misreported revenues. Of the remaining 73 percent, a majority misclassify their reported costs in a category other than professional fundraising fees, and 9 percent engage in cost allocations, meaning that not all telemarketing costs are reported as fundraising expenses. Using an even more conservative design that compared a single year of campaign revenue and expenses to the sum of three years of firm-wide contributions and fundraising expenses, 14 percent of this sample is misreporting revenues. Of the remaining

a series of revenue recognition and allocator regressions employing combinations of these variables. PROFACCT is consistently insignificant in the revenue and allocation tests using the single year “digitized dataset,” suggesting that our result may differ from Krishnan et al. (2006) due to sample differences.¹⁹ Therefore, we conclude that the insignificance of PROFACCT in these two tests is not due to a lack of power and high multicollinearity.

Liquidity and operating margin are other measures of nonprofit financial distress (Tuckman and Chang 1991).²⁰ Neither of these additional financial distress variables is a significant predictor in any of the misreporting tests. The inclusion of the variables does not substantially change the results of the regression analyses.

We include dummy variables for each year during the sample period. Our results do not change when these year variables are included in the regression. Additionally, we run separate regressions for each year in the sample period. Although, we find the incidence of misreporting decreases during the sample period, our initial results are robust across years.

Yetman and Yetman (2006) indicate that states may vary in the degree to which they regulate nonprofits. Therefore, we include dummy variables for each state in our sample. Our results do not change when these variables are included in the analysis.

As previously mentioned, we use a very conservative estimate of total telemarketing revenues and expenses, which decreases the likelihood that we will improperly classify a nonprofit as a net reporter, a misclassifier, or an allocator. In order to protect against improperly classifying an organization as a gross reporter, we create alternate measures of *REVDIFF*. Instead of classifying an organization as a net reporter if *REVDIFF* < 0, we label an organization a net reporter if total telemarketing revenues from all telemarketing firms are less than 95 (or 90) percent of reported contributions on Form 990. These alternative measures adjust for any organizations that may be net reporters, but appear to be gross reporters because they have large contributions from sources other than telemarketing. We create similar alternative measures of *FEEDIFF* and *FUNDEXPDIFF*, where in order to be categorized as a misclassifier or an allocator, the total telemarketing fees must exceed 95 (or 90) percent of total reported professional fundraising fees or total fundraising expenses on Form 990. The results are robust to the definitions of *REVDIFF*, *FEEDIFF*, and *FUNDEXPDIFF*.

Because the telemarketing reports do not indicate the type of service the professional fundraiser provides, our concern is that a limited number of the professional fundraisers in our sample conduct special events, such as selling tickets to a performance (Keating et al. 2003) and do not merely provide telemarketing services. In these instances, the IRS requires revenue generated from special events to be treated differently than contribution revenue. Thus, it is possible that the organizations report all or part of the “telemarketing” collections as special event revenue in Part 1 line 9a (gross revenues from special events). In this case, we could incorrectly classify an organization as a net reporter. Further, although IRS guidelines require that the “telemarketing” fee be reported on the professional fundraising fee line, the instructions on Form 990 on how to handle special event revenue and their related costs are ambiguous. Thus, some Form 990 preparers may interpret “direct expenses of special events” to include the fundraiser’s fee. If this is the case, then the preparer will

¹⁹ We also conducted analysis of variance influence factors, a test which can only be run on an OLS specification. It did not indicate any multicollinearity problems.

²⁰ A number of organizations have zero liabilities, which results in a current ratio that is undefined. We invert the current ratio to avoid losing observations. Further, we deflate profit margin by total assets rather than total revenue because revenue may be misreported.

virtually all firm-years in the second test are audited. Similarly, we find that the accrual variable is insignificant. The firms that spend on outside accounting or auditing services do not display significantly different allocation behavior from their counterparts that do not use outside services. However, we should note that we find that firms that have restricted funds are less likely to allocate some telemarketing costs to non-fundraising activities, but the statistical significance is marginal (significant at 0.10). Overall, the results suggest that accounting sophistication and certain monitoring activities have a substantial effect on the likelihood that telemarketing expenses will be allocated to expense categories other than fundraising expense.

Additional Tests

We conduct supplemental tests to examine the robustness of our results. First, we recognize that the telemarketing proceeds may be drawn from a state report whose fiscal year does not overlap precisely with the Form 990 fiscal year. For example, a yearly state report may cover a calendar year while a nonprofit has a different fiscal year. Moreover, a fundraising campaign by a telemarketer may start on July 1 and end on June 30 of the following year. For nonprofits with a calendar fiscal year, revenues generated by telemarketing firms would be reported in two separate Form 990 filings. Additionally, if a campaign is conducted across multiple years, the partial results of the first year of the campaign may be reported to the state in one year, with the entire campaign (gross receipts from years one and two) reported to the state in the subsequent years. There may also be numerical variation due to differences in the cash and accrual methods used (especially if a campaign generates pledges receivable in one year that is collected in a subsequent year).

To decrease the likelihood that we improperly label a nonprofit organization as misreporting due to the mismatch between the campaign and fiscal years, we create two additional datasets. We sum Form 990 data for a three-year period, including the year of the campaign and the years immediately before and after. For a nonprofit to be labeled as a net reporter or an allocator, it would then need to have three-year Form 990 numbers that are smaller than a single year of campaign data. Combined three-year datasets are constructed using financial information from both the Core and Digitized Data files ("both sources three-year") and from only the Digitized Data file in later years ("digitized data three-year"). We use these more conservative datasets to test the robustness of the regression results obtained using our first two datasets.

The results from regressions that use the "both sources three-year" dataset to examine net reporters are largely the same as those obtained from the single year "both sources" dataset and have a pseudo R^2 of 27 percent. The only difference is that *LEVERAGE* is not a significant predictor of either net reporting or the likelihood of allocating telemarketing expense out of fundraising expense. With the "digitized data three-year" dataset, we find largely similar results; however, *lnASSETS* and *LEVERAGE* are insignificant. The variables appear to be insignificant in the regression due to a low correlation with the dependent variable rather than a high degree of multicollinearity with other independent variables.

The three-year datasets also generate results for the allocator test that are qualitatively similar to the single year datasets. For the "both sources three-year" dataset, the 501(c)(3) variable is dropped from the regression because it is so highly correlated with the allocation decision. However, the other variables remain significant as in the single-year regression. With the "digitized dataset three-year," the results are essentially the same with one notable exception: *PROFACCT* is significant, while *ACCRUAL* is not.

A potential explanation for the shift is the high degree of multicollinearity between *ACCRUAL*, *PROFACCT*, *AUDIT*, and *RESTRICTED* in the Digitized Data sample. We ran

TABLE 9
Logistic Regression of Likelihood of Allocation of Fundraising Expense Using Two Datasets

$$P(\text{Allocator}) = \lambda_0 + \lambda_1 \ln \text{ASSETS} + \lambda_2 \ln \text{OFFSAL} + \lambda_3 \text{ACCRUAL} + \lambda_4 \text{PROFACCT} \\ + \lambda_5 \text{RESTRICTED} + \lambda_6 \text{501C3} + \lambda_7 \text{AUDIT} + \lambda_8 \text{LEVERAGE} \\ + \lambda_9 \text{YIELD} + \lambda_{10} \text{SECTOR} + \varepsilon$$

	Predicted Sign	Both Sources		Digitized Dataset	
		Coefficient ^a (Standard Error)	Marginal Effect ^b	Coefficient ^a (Standard Error)	Marginal Effect ^b
<i>lnASSETS</i>	—	−0.23*** (0.04)	−1.8%	−0.28*** (0.06)	−1.3%
<i>lnOFFSAL</i>	?	−0.07*** (0.02)	−0.6%	−0.07*** (0.02)	−0.3%
<i>ACCRUAL</i>	—			0.43 (0.36)	1.7%
<i>PROFACCT</i>	—			0.37 (0.25)	1.6%
<i>RESTRICTED</i>	—			−0.55* (0.31)	−2.8%
<i>501C3</i>	—	0.55* (0.29)	3.5%		
<i>AUDIT</i>	—	0.02 (0.30)	0.1%	−0.01 (0.40)	−0.1%
<i>LEVERAGE</i>	?	−0.19*** (0.07)	−1.5%	−0.30*** (0.08)	−1.4%
<i>YIELD</i>	—	0.00 (0.00)	0.0%	0.00 (0.00)	0.0%
Constant		1.41*** (0.54)		2.09*** (0.74)	
n		2,954		1,883	
Pseudo R ²		13%		19%	

*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively, two-tailed test.

^a The standard errors and associated z-statistics are adjusted for the intrafirm correlations arising from the inclusion of multiple years of observations (Huber 1967; Rogers 1993).

^b The marginal effect indicates the effect on the probability of net reporting of a one-unit change for continuous variables and a change from 0 to 1 for discrete variables when independent variables are evaluated at their means.

P(Allocator) = 1 if *FUNDEXPDIF* < 0, and 0 otherwise;

lnASSETS = natural logarithm of beginning assets;

lnOFFSAL = natural logarithm of officers' salaries and wages;

ACCRUAL = 1 if organization uses accrual accounting, and 0 otherwise;

PROFACCT = 1 if accounting fees > 0, and 0 otherwise;

RESTRICTED = 1 if permanent or temporarily restricted net assets > 0, and 0 otherwise;

501C3 = 1 if entity is organized as a 501(c)(3), and 0 otherwise;

AUDIT = 1 if entity meets the audit requirements of a state in which it operates or solicits funds or is subject to an A-133 audit, and 0 otherwise;

LEVERAGE = total beginning liabilities/total beginning assets; and

YIELD = combined net proceeds from telemarketing campaigns/combined gross proceeds from telemarketing campaigns.

TABLE 4 (continued)

Change Period	Predicted Sign	x = -1		x = 0		x = +1	
		Parameter Estimate	p-value	Parameter Estimate	p-value	Parameter Estimate	p-value
$PIN_{t-1+x} * DR_t$	?	-0.037	0.64	-0.023	0.74	-0.075	0.32
$PIN_{t-1+x} * RET_t$	-	0.085	0.28	-0.071	0.24	-0.076	0.19
$PIN_{t-1+x} * RET_t * DR_t$	+	0.075	0.89	0.856	0.02	0.887	0.06
Average Adj. R ²		0.20		0.20		0.21	
Average n		940		940		940	

This table presents the results of Fama-MacBeth regressions estimated annually over the 1985 to 2000 time horizon.

p-values are based on the time-series standard errors of the coefficient estimates, where the standard errors are corrected for autocorrelation using the Newey-West procedure. p-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise

NI_t = year t net income before extraordinary items (Compustat #18) divided by beginning of fiscal year t market value of equity;

RET = buy and hold return over fiscal year t ;

DR_t = 1 if RET_t is negative, and 0 otherwise; and

ΔPIN = annual change in the PIN score, where the PIN is the probability of an information-based trade.

with greater conservatism in the contemporaneous year, consistent with the H2c. None of the beginning of change period PIN score terms are significant suggesting that the change in the PIN score is subsuming the effect of the prior period PIN score on conservatism.

Columns three and four in Panel C of Table 4 present the results from estimating Equation (4) with the contemporaneous change in PIN . We expect contemporaneous increases in information asymmetry to be associated with increased conservatism due to the fact that increases in information asymmetries most likely occur throughout the year and thus may affect the contemporaneous level of conservatism. Consistent our prediction, the $\Delta PIN_t * RET * DR$ coefficient (β_7) is 0.965, significant at the 0.01 level, one-sided. The contemporaneous results differ from the prior period change in PIN results in that the $\Delta PIN_t * RET$ term is no longer significant and the $PIN * RET * DR$ term, the $t-1$ level of the PIN interaction term, is positive and significant.

The last two columns of Panel C of Table 4 test the third hypothesis by examining the influence of future PIN changes on contemporaneous conservatism. We find no support for the third hypothesis that greater conservatism leads to increases in information asymmetry. None of the coefficients on future change in PIN interaction terms are significant, inconsistent with greater conservatism in the contemporaneous period resulting in increases in information asymmetries in the future. However, consistent with the results reported in Table 2 we find that the $PIN * RET * DR$ term is positive and significant at the 0.06 level.

The results reported in Table 4 are inconsistent with recent moves by the FASB toward fair value accounting, away from conservatism, in an effort to reduce information asymmetry. An implication of the combined results reported in Table 4 is that moving toward fair-value-based accounting is likely to *increase* the level of *information asymmetry* among equity market participants.

As discussed in Section IV, the structure of the Basu model makes the estimation of changes in conservatism difficult. Therefore, we use the specifications presented in Table 4 as our main tests of H2 and H3. Presented in Table 5 are the results of one additional series of tests to provide further evidence on H2 and H3. Specifically, we examine the

TABLE 5
Changes in PIN Analysis and Changes in Conservatism
Fama-MacBeth Regressions

Panel A: Coefficient Estimates

$$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \varepsilon$$

	<u>Mean ΔPIN</u>	<u>Year $t-2$</u>	<u>Year $t-1$</u>	<u>Year t</u>	<u>Year $t+1$</u>
ΔPIN Group		$\beta_{3,t-2}$	$\beta_{3,t-1}$	$\beta_{3,t}$	$\beta_{3,t+1}$
Group 1	-0.073	0.517	0.461	0.589	0.653
Group 2	-0.017	0.351	0.441	0.410	0.703
Group 3	0.011	0.303	0.379	0.563	0.807
Group 4	0.069	0.524	0.514	0.553	0.777

Panel B: Within Group Test of Differences

	<u>Pre-Change Period Difference Year $t-1$, Year $t-2$</u>	<u>Concurrent Change Period Difference Year t, Year $t-1$</u>	<u>Post-Change Period Difference Year $t+1$, Year t</u>
ΔPIN Group	$\beta_{3,t-1} - \beta_{3,t-2}$	$\beta_{3,t} - \beta_{3,t-1}$	$\beta_{3,t+1} - \beta_{3,t}$
Group 1	-0.055 0.20	0.128 0.00	0.064 0.18
Group 2	0.091 0.08	-0.031 0.28	0.293 0.00
Group 3	0.076 0.13	0.184 0.01	0.244 0.02
Group 4	-0.010 0.44	0.039 0.22	0.224 0.00

Panel C: Across Group Test of Differences

	<u>Pre-Change Period Difference Year $t-1$, Year $t-2$</u>	<u>Concurrent Change Period Difference Year t, Year $t-1$</u>	<u>Post-Change Period Difference Year $t+1$, Year t</u>
	Group 4 ($\beta_{3,t-1} - \beta_{3,t-2}$) - Group 1 ($\beta_{3,t-1} - \beta_{3,t-2}$)	Group 4 ($\beta_{3,t} - \beta_{3,t-1}$) - Group 1 ($\beta_{3,t} - \beta_{3,t-1}$)	Group 4 ($\beta_{3,t+1} - \beta_{3,t}$) - Group 1 ($\beta_{3,t+1} - \beta_{3,t}$)
Group 4 - Group 1	0.045 0.31	-0.089 0.08	0.160 0.06

Panel D: Overtime Changes in PIN

$$NI = \alpha + \beta_1 DR + \beta_2 RET + \beta_3 RET * DR + \varepsilon$$

	<u>Mean ΔPIN</u>		
	<u>Year t</u>	<u>Year $t+1$</u>	<u>Year $t+2$</u>
ΔPIN Group			
Group 1	-0.073 0.00	0.029 0.00	0.000 0.68
Group 2	-0.017 0.00	0.006 0.00	-0.001 0.26
Group 3	0.011 0.00	-0.006 0.00	-0.001 0.29

(continued on next page)

TABLE 5 (continued)

	Mean ΔPIN		
	Year t	Year $t+1$	Year $t+2$
Group 4	0.069	-0.033	-0.001
	0.00	0.00	0.54

Panel A presents select Fama-MacBeth coefficients from annual cross sectional estimations of the Basu (1997) regression for each of the change in PIN quartiles, where PIN quartiles are formed by ranking firms into four groups based on the change in PIN over period t , (i.e., $PIN_t - PIN_{t-1}$).

Panels B and C test whether the time-series average of the difference in the coefficient estimates is different from zero. p-values are based on the time-series standard errors of the coefficient estimates, where the standard errors are corrected for autocorrelation using the Newey-West procedure. p-values are one-tailed.

Panel D tests whether the change in PIN is significantly different from zero, two-tailed.

NI = net income before extraordinary items (Compustat #18) divided by beginning of fiscal period market value of equity;

RET = buy and hold return over the fiscal year;

DR = 1 if RET is negative, and 0 otherwise; and

ΔPIN = annual change in the PIN score, where the PIN is the probability of an information-based trade.

timing of changes in conservatism before, during, and after the change in information asymmetry.

We first group firms into quartiles based on the change in the PIN score over the fiscal year (i.e., $PIN_t - PIN_{t-1}$). We then estimate Equation (1), the Basu model, for each of the quartiles for fiscal year's $t-2$, $t-1$, t , and $t+1$, providing an estimate of the level of conservatism for each of the quartiles in years surrounding the change in information asymmetry. We then examine the change in conservatism in the pre-change period, the concurrent change period, and the post-change period.

Panel A of Table 5 presents the average change in PIN for each of the quartiles as well as the Fama-MacBeth Basu coefficients (the β_3 coefficient from Equation (1)) for each of the quartiles and reporting periods. For the firms in the first (fourth) quartile the average ΔPIN_t is -0.073 (0.069). In general, conservatism is increasing over time as indicated by the general increase in the β_3 coefficient from $t-2$ to $t+1$ for each of the groups.

Panel B of Table 5 presents our first additional tests of H2 and H3. Specifically, within each change quartile we take the change in the Basu coefficient within the group for the pre-change period ($\beta_{3,t-1} - \beta_{3,t-2}$), the concurrent change period ($\beta_{3,t} - \beta_{3,t-1}$) and the post-change period ($\beta_{3,t+1} - \beta_{3,t}$). The second hypothesis, changes in information asymmetry lead conservatism, predicts that conservatism increases (decrease) in the post-change period for the sample of firms experiencing an increase (decrease) in information asymmetry in the contemporaneous period. The prediction from the third hypothesis, conservatism leads changes in information asymmetry, is that conservatism increases (decreases) in the pre-change period for the sample of firms experiencing an increase (decrease) in information asymmetry in the contemporaneous period.

Panel B of Table 5 presents the time-series average of the coefficient differences and the p-value testing whether this difference is different from zero. The results reported in the first column of Panel B provide no evidence consistent with the third hypothesis. The second quartile is the only group where the change in conservatism is significant. For this quartile conservatism increased by 0.09, p-value of 0.08, one-sided, opposite of what is expected since the mean change in PIN for this group is negative. The results reported

in the second column examine the change in conservatism during the change in PIN period, i.e., the contemporaneous change period difference. The first and third quintiles exhibit a significant increase in conservatism, 0.128 and 0.184, respectively. Thus, for the sample of firms with the largest PIN decrease (the first quartile), conservatism is increasing over the period in which information asymmetries are declining.

The third column of Panel B of Table 5 provides some evidence consistent with the second hypothesis. Specifically, there is a significant increase in conservatism following the change in information asymmetry for in both the third and the fourth quintiles, the groups where information asymmetries have increased. However, somewhat inconsistent with the second hypothesis, conservatism does not decline in the first quartile and actually increases significantly in the second quartile.

Panel C of Table 5 examines the across group differences in each of the three periods. Specifically, we focus on the two extreme quintiles, the fourth and the first, and test whether the change in conservatism across the quintiles is significantly different from zero. There is no difference in the change in conservatism across the groups in the pre-change period. In the concurrent and post-change period there is a significant difference in the change in conservatism between the fourth and first quintile. Conservatism increased significantly more in the first quintile relative to the fourth quintile in the concurrent period, as evidenced by the -0.089 difference in difference, p -value 0.08, one-sided. However, in the post-change period the change in conservatism in the fourth quintile is significantly larger than the change in conservatism in the first quintile, difference in difference of 0.16, p -value 0.06, one-sided. Overall the results presented in Table 5 are most consistent with the second hypothesis, changes in information asymmetry lead conservatism.

Finally, Panel D of Table 5 examines the change in *PIN* in year t , $t+1$, and $t+2$ for the *PIN* change groups. The first column of Panel D reports the results for the year t , the ranking year, changes in *PIN*. The second column of Panel D reports the results for the change in *PIN* in year $t+1$ for groups formed on the year t change in *PIN*. On average, the *PIN* decreases (increases) in year $t+1$ for groups of firms experiencing an increase (decrease) in *PIN* in year t . Specifically for the group of firms with the largest increase (decrease) in *PIN* in year t , the average *PIN* decreases (increases) by -0.033 (0.029) in year $t+1$. We also examine the change in *PIN* in year $t+2$ for the four groups, none of which are statistically significant. The combined results in Table 5 indicate that increases in information asymmetry lead to greater conservatism, which, in turn, leads to decreases in information asymmetry.

Together, the results presented in Tables 2, 3, 4, and 5 provide evidence consistent with the first and second hypotheses. Specifically, we find that higher levels of information asymmetry between managers and outside equity investors are associated with more conservative earnings and that increases in information asymmetry lead to greater conservatism. The combined results are consistent with our conjecture that conservatism is an equilibrium response by equity market participants to reduce agency cost arising from information asymmetries between managers and uninformed investors.²³

²³ Differences in firms' regulatory environments potentially influence the demand for conservatism (Watts 1977). To ensure that our results are not due to the effect of differences in firms' regulatory environments on conservatism we repeat the main analysis (Tables 2, 3, 4, and 5) after eliminating regulated industries (Financials and Utilities). The results of these analyses are consistent with the tabled results. We continued to find support for first and second hypothesis.

VI. ADDITIONAL ANALYSIS

Firm Size

As a robustness test we repeat the analysis presented in Table 5 including firm size and its interaction terms. We measure firm size as the natural log of firm's market value of equity at the end of the estimation period. There are at least three potential reasons to expect firm size to be related to conservatism: (1) political costs; (2) the effect of aggregating income and returns across multiple segments/projects; and (3) information asymmetry. The political cost hypothesis predicts that larger firms will report more conservative earnings, whereas the income aggregation and information asymmetries hypotheses predict that larger firms will report less conservative earnings.

In untabulated analysis, we find that larger firms are less conservative, consistent with income aggregation and information asymmetries. The inclusion of firm size and size interaction terms in the regression reduces the effect of the PIN on conservatism. In the one-, two-, and three-year specifications, the coefficient on the three-way interaction term, $PIN * RET * DR$, is positive but not significant at conventional levels. The results are not inconsistent with our hypotheses since one of the potential reasons for firm size influencing conservatism is information asymmetries.²⁴

Alternative Measures of Information Asymmetry

To provide further support for our hypotheses we repeat the analysis using the average bid-ask spread over the fiscal year. Specifically, we collect the closing bid and closing ask from the CRSP daily stock file. We then take the average of the bid-ask spread scaled by closing price over the fiscal year. We repeat the entire analysis using the average bid-ask spread and the change in the bid-ask spread. The results of this additional analysis, available upon request, are consistent with the tabled results using the PIN score. Specifically, we find that firms with higher bid-ask spreads report more conservative earnings and that changes in the bid-ask spread lead changes in conservatism.

Non-Operating Accruals

As a final robustness test we examine the association between the PIN score and non-operating accruals, where non-operating accruals (*NONOP*) are defined as net income (Compustat #172) plus depreciation and amortization expense (Compustat #14) minus cash flow from operations (Compustat #308) plus the change in inventory (Compustat #303) plus the change in accounts receivable (Compustat #302) plus the change in accounts payable (Compustat #304) plus the change in accrued income taxes (Compustat #305), scaled by total assets (Compustat #6). Requiring statement of cash flow data to calculate *NONOP* significantly reduces the sample (average of 493 firms a year) and time period (1988–2001). For each fiscal year from 1988 to 2001 we estimate the following regression:

²⁴ The results indicate that larger firms report less conservative earnings. As suggested above one potential reason for this result is the aggregation effect. To provide some evidence on the aggregation hypothesis, we include an additional dummy variable in Basu's model, *MULTSEG*, equal to 1 if the firms reports more than one business segment for the fiscal period, and 0 otherwise. We use the *MULTSEG* dummy to capture cross-sectional differences in the aggregation effect. We expect firms with multiple segments to have relatively more aggregation in both returns and earnings thus reducing conservatism. The untabulated results indicate that *MULTSEG* dummy is not associated with conservatism and does not affect the influence of *SIZE* on conservatism. The combined results provide some evidence that the effect of *SIZE* on conservatism is unlikely to be entirely driven by the aggregation effect. We interpret the results as providing further evidence that the negative association between firm size and conservatism is at least partially due to firm size proxying for information asymmetry.

$$NONOP = \alpha + \beta_1 PIN + \epsilon \quad (8)$$

where all variables are as previously defined.

In untabulated descriptive statistics the mean non-operating accrual is -0.016 for our sample. The Fama and MacBeth (1973) coefficient on PIN is -0.033 , significant at the 0.01 level, one-sided. This result in conjunction with the descriptive statistics on $NONOP$ indicates that firms with larger PIN scores report more negative non-operating accruals providing further evidence that firms with larger PIN are more conservative. In addition this result provides some evidence on the accounting mechanism through which firms with larger PIN scores are more conservative.

VII. CONCLUSION

In this paper we investigate the effect of information asymmetries between managers and outside equity investors for conservatism. We find that firms with higher PIN scores incorporate less information about gains and more information about losses into the current financial statements. In addition, we find that the asymmetric recognition of gains and losses in the current financial statements is increasing in the level of information asymmetry. Our results are consistent with equity investors demanding more conservative earnings as a means of mitigating agency problems. Moreover, we find that conservatism increases following increases in information asymmetries. This result further supports our contention that conservatism is an equilibrium response to mitigate value reductions resulting from information asymmetries between managers and outside equity investors.

We interpret our results as consistent with our argument that users of accounting information value the verifiability of accounting information, in that accounting provides a hard, verifiable benchmark useful in mitigating information asymmetries and the resulting agency cost. In settings with higher information asymmetries the role of accounting is not to report equity values, but instead to provide verifiable information useful for assessing alternative information sources on equity values and mitigating agency cost. Our results have implications for accounting standard-setters that attempt to eliminate conservatism in financial reports. Our finding that conservatism increases in response to increases in information asymmetries in equity markets provides the first evidence, we are aware of, that contradicts the FASB's view that conservatism limits the information role of financial statements in equity markets. Further, our findings highlight that conservatism is an equilibrium response to mitigate value reductions arising from information asymmetries.

Finally, our results have implications for accounting research examining the determinants of conservatism. Prior research tends to focus on the demands for conservatism from parties entering into explicit contracts with the firm (e.g., debt and compensation contracts). Our results confirm the strong influence of debt contracting on conservatism, but also indicate that governance mechanisms (e.g., control of management via conservatism) generate a demand for conservatism much like that generated by explicit contracts.

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The Association between Technological Conditions and the Market Value of Equity

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ABSTRACT: The objective of this study is to provide evidence on how technological innovation conditions underlying the firm's investments drive earnings growth and, hence, market value of equity. Technologies develop and flourish or die out through the combined investment decisions of those firms doing the inventing, and those firms that adopt those inventions, and thereby help to spread (or diffuse) the innovations into wider use. Hence, technology is important for the investment decisions of all firms, regardless of whether they patent. We focus on three aggregate measures of technological innovation conditions: the success rate of past technological investments, technology complexity, and the technology development period. We use the interactions between each of these three conditions with earnings to capture the combined effect on market value of a firm's technological innovation environment. Our sample comprises 12,594 U.S. firm years for the period 1990–2000 including firms actively producing new technologies and firms that adopt technologies for their processes and products. Our primary and additional tests suggest that the interactions capture value-relevant information not reflected in commonly used variables including industry, research and development, sales, general, and administration expenses, risk, and growth. We also triangulate our results by providing evidence that aggregate technological innovation conditions predict future earnings and are, hence, instrumental in the earnings-generation process. This paper extends the valuation literature by (1) developing a generalizable framework that explains how technological innovation conditions link to future earnings and therefore map into the market value of equity; (2) developing

The authors gratefully acknowledge Dan Dhaliwal (editor), the anonymous referees, and comments of Bob Bowen, David Brown, Greg Clinch, Jeff Coulton, Elizabeth Demers, Peter Easton, Jere Francis, Wayne Guay, S. P. Kothari, Baruch Lev, Jim Ohlson, Ken Peasnell, Stephen Penman, Gordon Richardson, Terry Shevlin, Dan Simunic, Doug Skinner, Nasser Spear, Don Stokes, Shiva Rajgopal, Stephen Taylor, Jon Tyler, Peter Wells, Steve Young, and participants of the Accounting and Finance Association of Australia and New Zealand Conference (2003), European Accounting Association Conference, Copenhagen (2002), and workshops at New York University, University of Lancaster, Oxford University, University of Western Australia, University of Technology, Sydney, University of Washington, and the Symposium on Capital Market Research at the University of Melbourne (2002). Thanks to André Gygyax for assistance with the data. Professor Wyatt thanks the Intellectual Property Research Institute of Australia at the University of Melbourne for their support.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted March 2006

Accepted August 2007

aggregate measures of technological innovation conditions that are relevant for estimating future earnings and value for *all* firms; and (3) providing detailed empirical evidence on the relation between these aggregate measures and the market value of equity and earnings for all firms not just those that patent.

Keywords: *technological innovation conditions; market value of equity; technology success rate; technology complexity; technology development period; earnings.*

Data Availability: *Data used in this paper are available from commercial data providers.*

I. INTRODUCTION

The relation between accounting earnings and the market value of equity is well documented and accepted in the professional and academic communities (e.g., Penman 2004; Palepu et al. 2004). However, the information sources that provide insights on the earnings-generation process for the purpose of equity valuation are much debated. Previous studies have examined the associations between market value and various cash flow and accounting items including the book value of equity, earnings, and components of earnings. More recently, researchers have extended the search for value-relevant information related to earnings growth to firm-specific nonaccounting information. Examples include "POPS," "page clicks," and "eye balls" (e.g., Amir and Lev 1996; Cheng 2003; Trueman et al. 2000; Hand 2000; Demers and Lev 2001; Bartov et al. 2002). To date, the relations between equity value and non-firm-specific information that interacts with earnings to produce future earnings and value have received limited attention.

In this paper, we address this gap in the accounting literature. We examine how aggregate technological innovation conditions, relating to technology areas, inform earnings innovations and thereby map into the market value of the firm's equity.¹ Our key motivation is that the previous research utilizes firm-level measures of technological innovation conditions to explain the market value of equity. However, these firm-specific measures are only available for those firms that patent.² All firms make technological investments but not all firms patent. In fact, the most common mechanism for appropriating benefits from investment is trade secrets.³ According to Schumpeter's (1934, 1939, 1942) theory of economic change, technological innovation is important for all firms' investment decisions, as either a determinant of the firm's decisions about inventive activities, or as a determinant of the firm's technology-adoption decisions relating to products and processes (e.g., Grenadier and Weiss 1997; Hansen and Serin 1997). That is, investment in technological innovation is not the exclusive domain of the high-intensity R&D firms or the prolific patenters (e.g., Audretsch 1995). Instead, technologies develop and flourish or die out

¹ A technology area is not an industry but an area of innovation based on the International Patent Classification System (see the Appendix). A given technology area has applications across multiple industries.

² In our sample of 12,594 firm years, only 14.9 percent of the firm years (about 1,897 firm years) are actively patenting into the United States patent system.

³ Cohen et al. (2000) find that firms protect profits from their innovation activities with a range of mechanisms including patents, secrecy, lead time advantages, and complementary use of marketing and manufacturing capabilities. Secrecy is the most common mechanism employed. They find patents are not generally used for direct protection of profits. For example, patents are used for blocking related innovations, to force rivals into negotiations, and to prevent law suits. More generally, the range of mechanisms available goes beyond patents to include trademarks, industrial designs, plant breeder rights, and business, company and domain names; as well as automatic rights under legislation and common law such as copyright and circuit layout rights, and commercial strategies such as trade secrets and confidentiality agreements www.ipaustralia.gov.au/strategies/X_strat.htm.

through the combined investment decisions of those firms doing the inventing, and those firms that adopt those inventions and thereby help to spread (or diffuse) the innovations into wider use. Hence, technological innovation is important for the investment decisions of all firms regardless of whether they patent.

Our contention is that the theory of innovation and economic growth can guide us in developing aggregate measures of technological innovation conditions that are applicable to all firms, not just research and development (R&D)-intensive firms or firms that patent. This approach is in contrast to the prior studies that focus on measures only relevant for subsets of firms that patent. For example, Deng et al. (1999) relate firm-level patents granted and patent citation data to stock prices for firms in the R&D-intense chemicals, drugs, and electronics industries. As a follow up to Narin (1995), Hall et al. (2001) provide descriptive evidence of an association between Tobin's Q and R&D, patents, and citation-weighted patents at the firm level. Jaffe (1989) examines the association between macroeconomic technology areas and R&D investment.⁴ These studies have made major contributions to our understanding of how to measure technological activities (see Patel and Pavitt 1995). We extend this literature by (1) developing a generalizable framework that explains how technological innovation conditions link to future earnings and, therefore, map into the market value of equity for all firms; (2) developing aggregate measures of technological innovation that are relevant for estimating future earnings and value for all firms; and (3) providing detailed empirical evidence on the relation between these aggregate measures and the market value of equity and earnings for all firms as well as a subsample of firms that do not patent.

We focus on three key technology conditions that we identify from the economic literature: (1) the success rate of previous investments in a technology area; (2) the complexity of the underlying knowledge base in a technology area, and (3) the average technology development period, which is the length of time required for a technological innovation to become profitable. These constructs are measured at the technology area level, within the firm's industry, using patent counts, patent citation, and science (paper) bibliographic data from a commercial database.

The results of our tests based on 12,594 firm years for 1990–2000 suggest that the interactions of current earnings with each of the three separate technology conditions, success rate, technology complexity, and technology development period, are associated with market value. Additional tests confirm that our results are robust to alternative specifications. In particular, when earnings interactions with SIC industry codes, and risk and growth measures are included in the regressions, the coefficients on the earnings-technology condition interactions retain the predicted signs and significance. When research and development costs and sales, general, and administration expenses are included as control variables, our results remain the same. Moreover, when we replace earnings in the interaction terms with estimates of accruals and cash flows, the previously documented bias in their persistence estimates disappears, consistent with omitted variables relating to investment. Finally, we triangulate our results in two ways. First, we test the relation between future realized earnings and the three key technology conditions. We find a positive (negative) relation between technology success rate and technology complexity (technology development period) and future earnings consistent with technology conditions playing an important role in the earnings-generation process. Second, we rerun the hypothesis tests for

⁴ See the review by Griliches (1990) of patent metric-based measures of technological innovation.

the subset of non-patenting companies in our sample and find evidence consistent with our hypotheses for these non-patenting firms.

Our study makes several contributions to the literature. First, it contributes theory and evidence to the valuation literature on how technology conditions are incorporated into the market value of equity. The theoretical valuation model of Ohlson (1995) and Feltham and Ohlson (1995) suggest that the value of a firm is determined by the book value of equity, a measure of earnings (or abnormal earnings), and other growth factors. However, the theory is silent on the identity of the other factors. Our paper identifies key growth factors from the economics literature. Because the technological innovation processes underlie all economic activity, these growth factors are qualitatively different from firm-specific variables. We provide evidence on how these non-firm-specific information sources are instrumental for estimating earnings and the market value of equity. Our paper therefore extends the valuation literature beyond firm-specific, nonfinancial variables.⁵

Second, our study suggests an approach to evaluate future earnings for intangibles-intensive companies.⁶ Amir and Lev (1996), Boone and Raman (2001), and others suggest valuation and cost-of-capital distortions arise for intangibles-intensive firms due to uncertainty associated with these types of investment and the current inability of accounting to comprehensively capture this investment.⁷ Our framework, which articulates the links between key technology conditions and earnings innovations, and hence value, suggests empirical proxies that capture investment returns missing from the accounting data. Moreover, the wide availability of patent office data to measure technology conditions also renders the theory developed in our paper empirically tractable.

The remainder of this paper is organized as follows. The next section presents a detailed discussion of the theory of the economics of innovation, and how our hypotheses and tests are derived from that literature. Section III describes the sample selection, data, and research design. The results are analyzed in Section IV and Section V presents our conclusions.

II. THEORY AND HYPOTHESES DEVELOPMENT

The theory framework is based on Schumpeter's theory of economic growth (1934, 1939, 1942). Schumpeter proposed that the economy is driven by the invention of new technology and the availability of short-term monopoly rents.⁸ Monopoly rents relates to the firm's capacity to obtain property rights over their investments and appropriate the expected profits.

Schumpeter argued that technological opportunities and expected profits motivate investment. The knowledge generated by these investments accumulates over time to form distinct technology "clusters" or "technology areas" (Nelson and Winter 1977). The current states (or conditions) in a technology area reflect the cumulated history of all investments

⁵ For example, Amir and Lev (1996); Cheng (2003); Trueman et al. (2000); Hand (2000); Demers and Lev (2001); Bartov et al. (2002).

⁶ Several studies provide evidence of firms' unmeasured innovation efforts: e.g., Klemperer (1995) examines consumer switching costs; Trajtenberg (1991) studies patents in the optical scanners industry; and Klock and Megna (2000) study advertising, R&D, radio spectrum licenses, and measures of the firm's customer base.

⁷ Similarly, Nakamura (2001) estimates the U.S. intangible investment at the macroeconomic (national accounts) level in 2000 was \$1 trillion. Only \$250 billion of this is included in the macroeconomic "Research and Development" account. The remaining \$750 billion intangible investment is not observable.

⁸ Griliches (1987, 1010) describes a technology area as "the average set of recipes for doing things, embedded in the current range of equipment and in the training of the existing workforce, and the currently known best way of converting [these] resources into outputs desired by the economy."

in technological, organizational, and managerial innovations relating to that specific technology. These technology areas have different boundaries from and multiple applications across, industries, as illustrated in the Appendix.⁹

Schumpeter distinguishes three stages of technology development: invention, innovation (commercialization), and diffusion of new knowledge (other firms mimic the idea). These three phases make up the technology development period, the length of which is a condition that varies among the technology areas. Also varying across technology areas is the strength of the knowledge base and technological innovation opportunities attributes that we label the prior success rate. A further condition varying across technology areas is the complexity of the knowledge base that impacts the underlying complexity of the firm's investments and ability to appropriate expected earnings. Complexity depends on whether the technology area is advancing more on the basis of scientific than technical knowledge. Information about these technology conditions is endogenous to the firm's investment decisions (e.g., Audretsch 1995; Grenadier and Weiss 1997). We therefore expect this information to interact with accounting earnings to determine a firm's market value of equity.

Our theory assumes that the expectations of the marginal investor determine the market value of equity (e.g., Bartov et al. 2000). One example of a marginal investor is an analyst who works for an investment fund and specializes in an industry. The marginal investor uses widely disseminated public information that has been codified and "informal" information that is shared among those active in the technology area. Their expectations are formulated using "appropriate" technological and financial knowledge. We define "appropriate technological knowledge" to include both tacit and universal knowledge (Polanyi 1967). Tacit knowledge, an intangible factor of production, is "ill-defined," uncoded, and unpublished information (Dosi 1988). The ability to obtain and understand tacit information separates the expertise and superior analysis of the industry expert from the public. Universal knowledge is well-documented and publicly available information. Because marginal investors use both types of information to estimate future earnings, both are reflected in the market value of equity.

Our hypothesis development now focuses on the variation in three types of conditions in technology areas: (1) the past success rate of technological investments in a technology area; (2) the complexity of the technology; and (3) the length of the technology development period.

Success Rate of Previous Technological Investments

Success rate relates to the past technological innovation activities in a technology area. The past success rate refers to two factors: the strength of the knowledge base in throwing up commercial applications for a technology, and the past success in solving the technical problems that stand in the way of commercialization. This success rate condition is a culmination of the history of all past investment decisions associated with a technology (e.g., Nelson and Winter 1977; Saviotti and Metcalfe 1984). The past success rate relates

⁹ Olsson (2006, 7) provides an example. A process for creating a sustained vacuum was invented by a student of Galileo (1564–1642), which led to the realization that the atmosphere existed. This vacuum discovery was outside the technology of the time and it was not until 1712 that Thomas Newcomen found an empirical use for the new technology. He invented an atmospheric engine. Later in 1769, James Watt was granted a patent for a steam engine based on the same principles as Newcomen's engine but with refinements. This steam engine then opened technology opportunities in several industries and led to major investments and growth in transportation, mining and textiles.

to, and determines, the difficulty and value of current investment opportunities in the technology area. Past scientific and technical successes create current technological investment opportunities.

Not all firms invest in the same technology area. More specifically, firms have different capabilities that affect their ability to recognize and take advantage of investment opportunities (Dosi 1988). Firms need the relevant in-house capabilities to assimilate external knowledge and apply it to achieve commercial ends (Cohen and Levinthal 1990, 128).¹⁰ According to those authors, this capability is a function of prior investment in the technology area that generates up to date expertise in the area. Further, firms need managers with the internal knowledge to be able to adapt their strategic responses to the changing business environment (Penrose 1959). These managers take time to develop, a situation that is exacerbated by the shortage of skilled labor (Balconi 2002). Differences in the firms' capabilities mean that not all firms will be in a position to identify and take advantage of the opportunities in higher success rate technologies.

For firms with the requisite capabilities, investment is attractive when past successes in a technology area generate an active supply of opportunities from a strong knowledge base, and there is expected demand for the products arising from the technology (e.g., Astebro and Dahlin 2003). Jaffe (1989) provides corroborating evidence that firms modify their level of investment in response to technology opportunities. He defines technological opportunity as variation in the cost and difficulty of innovation in different technology areas and operationalizes the construct using the United States Patent Office classification of technology classes. Astebro and Dahlin (2003) find technology opportunities are a more powerful motivator of investment than expected demand, explaining about 1.5 times more of the variance in commercialization of inventions. Grenadier and Weiss (1997) model the optimal technological investment as a function of lower uncertainty about the potential, cost, and technical difficulties of the opportunities. Lower uncertainty of the latter types increases the predictability of future earnings.

We argue the marginal investor evaluates the future earnings implication of the firm's current earnings interactively with (in the context of) the strength of the knowledge base and technological opportunities, which we label the success rate. Given higher success rate technologies are subject to lower economic uncertainty, more predictable earnings, and lower risk for investors, we predict a positive relation between the market value of equity and the interaction between current earnings and success rate.

H1: There is a positive relation between the market value of equity and the interaction between a firm's current earnings and the success rate of previous technological innovations in the relevant technology area.

Technology Complexity

The knowledge base of a technology area may be growing due to advances in technology or advances in science. Once the knowledge base is advanced (through technology or science), there are incentives for firms to take advantage of the resulting investment opportunities and invest in these technologies. However, the firms face an "appropriability" problem. Specifically, because knowledge creation resides in people who cannot be owned,

¹⁰ This is one reason why outsourced R&D is problematic. Given the highly differentiated nature of technological knowledge, firms are selective in their innovation investment choices: "Firms will seek to improve and diversify their technology by searching in zones that enable them to use and to build on their existing markets, distribution arrangements [and as a consequence], what the firm can hope to do technologically in the future is narrowly constrained by what it has been capable of doing in the past" (Dosi 1988).

or exists in a form that is easily copied by rivals, the ability of the firm to protect its investment from rivals impacts the firm's ability to appropriate the profits from its investments (Webster 1999).

Many institutional and market-based factors in the economy assist firms to protect their investments from appropriation by competitors. Some of these include:

- Distribution, advertising, and marketing activities that generate brand name and market share advantages;
- The legal system for statutory and common law rights (e.g., telecommunications regulations, trade restriction contracts, licenses);
- Regulations that define and protect property rights (patents, trademarks, design, trade secrets, copyright);
- Subsidies and co-operative R&D ventures; and
- Nominations of national "champions" (high-profile individuals who or government bodies that can cut the red tape for innovating companies).

An additional factor is the complexity of the knowledge base. The most complex knowledge is (usually) scientific knowledge. The complexity of scientific knowledge provides firms with some protection from imitation and appropriation of profits (Callon 1994). Assimilating and commercializing scientific advances requires specialized training and skills, which renders this type of knowledge more costly for rivals to use (Callon 1994; Stephan 1996). Scientific knowledge is difficult to master because it includes a larger proportion of tacit knowledge, and a smaller proportion of well-documented and publicly available universal knowledge (Polanyi 1967; Dosi 1988; Metcalfe 1995, 455–462). Further, the demand for scientists exceeds the supply. This scarcity of experts with the complex tacit knowledge to invent or adopt science advances increases the costs of imitating or in some way using this knowledge (Balconi 2002).

Moreover, the reward systems for science advances provide defined property rights over the science innovations. Rewards have their basis in the "priority of discovery" (Merton 1957). These priority-based rewards are distinct from the reward systems that operate for the invention or adoption of technological advances, which are predominantly financial in nature (Stephan 1996). Firms have relatively more control over scientific advances compared to technical advances, and are more able to appropriate expected profits from their investments.

Overall, while property rights may reduce the risk of earnings appropriation by rivals, this risk cannot be eliminated. We propose that the marginal investor, who is an industry expert, takes account of the net effect of the prevailing appropriability conditions. Specifically, based on the protection afforded earnings from investments in complex technologies, and the property rights afforded by the reward systems for science, we hypothesize a positive relation between the interaction of current earnings and technology complexity, and the market value of equity.

H2: There is a positive relation between the market value of equity and the interaction between current earnings and technology complexity.

Technology Development Period

The technology development period refers to the average time required for a firm to transform its investments into a commercial application.

Longer development periods involve long duration projects and investments with a long delay until there is a flow of profits. There are long periods during which investments are

yet to be developed to an exchangeable form (i.e., not embodied in plant or inventory). Hence, there are long periods in which no formal property rights are available and therefore considerable appropriation risk.

Shorter development periods are characterized by intensifying competition and appropriation risk as firms vie to be first to market (e.g., Datar et al. 1997). Competition increases costs as the firm expedites the development or production process to keep ahead of competitors (Scherer 1966; Graves 1989).¹¹ However, compared to longer development periods, firms have a lower exposure to random shocks and technological obsolescence (Grenadier and Weiss 1997).

At a first pass, these characteristics suggest different types of risks but no dominant development period effect for expected earnings and value. However, researchers and firms do claim the shorter development period has a significant effect on the firm.¹² There is the practical issue of solving technical product and process problems in a short time period. Despite this constraint, some firms do succeed in problem solving as exemplified by the fact that development periods on average are not increasing but declining in length (Agarwal and Gort 2001). More fundamentally, a characteristic of shorter development periods is a closer proximity of the investment to future earnings. The shorter prediction period results in a shorter lag to positive earnings compared to longer development periods (i.e., if positive earnings were not expected within the average investment horizon, then marginal investors would exercise their abandonment option). This phenomenon is descriptive, on average, irrespective of the competitive risks of shorter cycles outlined earlier. This implies a negative association between the technology development period and future earnings, on average.

Based on these arguments, we predict a negative relation between the market value of equity and the interaction between current earnings and the technology development period. Specifically, as current earnings increases, the development period is conditionally negatively associated with the market value of equity, reflecting the shorter prediction period and marginal investor's expectation of positive earnings on average within the firm's investment horizon. Conversely, as the development period increases in length, current earnings is conditionally negatively associated with the market value of equity, reflecting the fact that the current earnings will eventually grow on average in the (possibly distant) future as the technology reaches feasibility and is embodied in products (i.e., otherwise investors exercise their abandonment option). We therefore predict a negative association between the interaction of current earnings and the development period and the market value of equity.

H3: The firm's market value of equity is negatively associated with the interaction between current earnings and the technology development period.

¹¹ Examples of competencies that enhance performance in shorter technology periods are the use of cross-functional teams, advanced design tools, customer involvement in the process, and product quality (Ittner and Larcker 1997).

¹² For example, see the management comments reported in Footnote 29 of Aboody and Lev (1998) relating to the negative influence of the shortened time of the software development cycle. Agarwal and Gort (2002) also provide evidence that firm survival is dependent on both the product and the firm life cycles, where the dynamics of both of these cycles comes from technological innovation in technology areas. We study the first order effect, which is the speed of development of new knowledge in a technology area (i.e., technology development period). Agarwal and Gort (2001) link the following factors to the development period of products: mobility of skilled labor, communication technology improvements, increases in the speed technical information spreads through the economy, an increasing population of potential entrants, and the increasing size of markets.

III. DATA AND RESEARCH DESIGN

Sample

The initial sample comprised Compustat firms classified in the following primary SIC code industries: Chemicals, Petroleum and Coal Products, Industrial Machinery and Equipment, Electronic and Electric Equipment, Transportation Equipment and Products, Instrument and Other Manufacturing, Communication, and Services. We included a range of, but not all, industries in the selection of the sample to balance the practical constraints of manually coding each individual firm-year with technology data while enabling the generalizability of the theory to be evaluated.¹³ We included all firms from these industries with the required data on Compustat and CRSP in the initial sample.

Our final sample includes firm-years with positive earnings and book value of equity. Before deleting firms with negative earnings and book value of equity, the sample of firm-years with the data required for our primary tests comprised 23,433 firm-years for 1990–2000. The final sample of 12,594 firm-years with the required data for the primary tests includes those firm-years with positive earnings and book value of equity for 1990–2000. Table 1 summarizes the sample composition by the primary SIC codes.

Table 2 shows the sample categorized by technology area. This technology area classification is a simplified version of the Intellectual Property Classification (IPC) produced by the World Intellectual Property Organisation (WIPO), and adopted by CHI Research (from whose databases we obtained our technology condition measures as discussed in detail below).

Table 2 indicates that our sample includes firms in all technology areas except miscellaneous machinery (technology area 20), and shows considerable variation in the number of firms across the areas. For example, in the Office Equipment and Cameras area, there are seven firm-years (0.06 percent of our sample); on the other hand, two categories together include almost a third of the firm-years—Telecommunications has 2,294 firm-years (18.22 percent) and Computers and Peripherals has 1,713 firm-years (13.60 percent).

To illustrate the variation across the technology areas in investment and product attributes, Table 2 shows the sample firms classified into the Organisation for Economic Co-operation and Development (OECD 2003) high to low technology classification. The OECD (2003) computes the categories using the expenditures for research and development and the extent of the value added to products. Table A2 in the Appendix explains the OECD classification. This classification yields the following groupings:

High technology sectors—pharmaceuticals, computers and peripherals, telecommunications, semiconductors and electronics, electrical appliances and components, and aerospace and parts;

Medium-high sectors—chemicals, biotechnology, medical equipment; medical electronics, industrial machinery and tools; office equipment and camera, motor vehicles and parts; other transport; and

Medium-low and low technology sectors—agriculture, oil and gas, mining, food and tobacco; textiles and apparel, wood and paper; primary metals, fabric, metals, and plastics, polymers and rubber; glass, clay and cement.

¹³ Standard & Poor's assigns individual companies to a four-digit Primary SIC Code by analyzing the firm's product line breakdown. The product line accounting for the largest percent of sales determines the Primary SIC Code. Each year this classification is reviewed. As a result, the sample includes firm-years in additional SIC code industries to the initial industries, as some firms are recoded to a different SIC in the 1990–2000 period.

TABLE 1
Sample Composition by Primary SIC Industry Codes

Industry	Primary SIC Codes	# Firm Years	% of Obs.
Agriculture, Energy, and Waste Services	1-999, 4900-4999	123	0.98
Mining and Construction	1000-1799	131	1.04
Food, Textiles, and Apparel	2000-2399	78	0.62
Wood and Paper Products, Furniture, Printing, Publishing	2400-2799	171	1.35
Chemicals, Petroleum, and Coal Products	2800-2999	1698	13.52
Rubber, Plastics, Stone, Clay, and Glass	3000-3299	129	1.03
Primary and Fabricated Metals	3300-3499	309	2.46
Industrial Machinery and Equipment	3500-3599	2109	16.78
Electronic and Electric Equipment	3600-3699	2615	20.82
Transportation Equipment and Products	3700-3799, 4000-4700	881	7.00
Instrument and Other Manufacturing	3800-3999	2114	16.62
Communication	4800-4899	703	5.59
Non-Durable, Building Material, and Merchandisers	5000-5999	371	2.94
Services	6000-8399	1162	9.25
	Total	12594	100.00
	Mean	900	7.14

The final sample comprises 12594 firm-years listed in the 1990-2000 period with positive earnings and positive book value of equity. Firms do not have to be listed for the entire 11 years to be included in the sample.

Initial sample selection was all firms in the following Primary SIC Code industries: Chemicals, Petroleum and Coal Products, Industrial Machinery and Equipment, Electronic and Electric Equipment, Transportation Equipment and Products, Instrument and Other Manufacturing, Communication, and Services. Standard & Poor's assigns individual companies a four-digit Primary SIC Code according to the largest percent of sales from the firm's product line breakdown. Standard & Poor's reviews the assigned classification each year when the company is updated. As a result, there are additional SIC code industries in addition to the selection industries above as the firm-years are recoded to a different SIC over the 1990-2000 sample period.

This diversity in our sample permits us to test our hypotheses, and provides assurance that our results apply across a wide range of firms in the economy.

Data Source and Variable Measurement

The individual firm-level stock prices and financial statement data were obtained from the CRSP and Compustat databases. The stock market data are the closing prices for the trading day occurring three months after balance date. The financial statement data are for fiscal year-end. The technology condition data are annual data.¹⁴

To test our hypotheses, we require measures of the three technology conditions—success rate, technology complexity, and technology development period—measured at the level of the technology areas. We obtain the technology data from a commercial supplier

¹⁴ The technology data are not strictly synchronous with the accounting data. We do not believe that this is fatal to our purpose for the following reason. At the technology-area level, the technology data is a measure of the accumulation of all prior economic activity in the area (up to the measurement period), which provides a snapshot of prevailing conditions. Given the length of the history that is embodied in the measures (i.e., the life of the technology to date); it is unlikely that the data is so precise that a lag of no more than six months would induce a "look ahead" bias. However, we acknowledge this as a potential limitation.

TABLE 2
Sample Firm-Year Distribution across Technology Areas

Technology Area (aggregated across all 26 CHI industries)	Technology Level (see Appendix)	# Firm- Years	Firm-Years % of Sample	# Firms 1990	# Firms 1991	# Firms 1992	# Firms 1993	# Firms 1994	# Firms 1995	# Firms 1996	# Firms 1997	# Firms 1998	# Firms 1999	# Firms 2000
1 Agriculture	Low	281	2.23	15	18	20	27	29	29	30	36	32	20	25
2 Oil and Gas	Low	238	1.88	12	11	16	19	21	21	26	36	28	26	22
3 Mining	Low	118	0.94	7	8	9	10	12	12	14	14	15	10	7
4 Power Generation and Distribution	Medium-low	57	0.45	2	3	3	6	7	6	6	7	6	6	5
5 Food and Tobacco	Low	35	0.28	3	3	3	4	5	2	3	5	4	2	1
6 Textiles and Apparel	Low	89	0.71	5	5	9	9	10	10	9	8	9	8	7
7 Wood and Paper	Low	381	3.03	32	28	29	31	38	42	41	42	37	33	28
8 Chemicals	Medium-high	334	2.65	22	29	27	36	36	30	37	32	35	27	23
9 Pharmaceuticals	High	555	4.41	25	37	40	45	49	51	55	67	62	63	61
10 Biotechnology	Medium-high	276	2.19	10	18	22	23	24	31	28	33	36	29	22
11 Medical Equipment	Medium-high	935	7.42	61	86	89	91	99	96	96	93	79	76	69
12 Medical Electronics	Medium-high	380	3.02	25	31	32	35	35	38	37	38	42	36	31
13 Plastics, Polymers and Rubber	Low	102	0.81	5	8	11	11	11	12	11	10	8	8	7
14 Glass, Clay and Cement	Low	81	0.64	4	5	7	7	10	12	13	7	6	5	5
15 Primary Metals	Low	12	0.09	0	0	0	1	1	1	1	2	2	2	2
16 Fabricated Metals	Low	827	6.57	55	51	64	70	79	91	89	91	88	81	68
17 Industrial Machinery and Tools	Medium-high	1	0.01	0	0	0	1	0	0	0	0	0	0	0
18 Industrial Process Equipment	Medium-high	227	1.80	14	22	19	21	22	24	26	23	19	19	18
19 Office Equipment and Cameras	Medium-high	7	0.06	0	0	0	0	1	1	1	1	1	1	1

(continued on next page)

TABLE 2 (continued)

Technology Area (aggregated across all 26 CHI industries)	Technology Level (see Appendix)	# Firm- Years	Firm-Years % of Sample	# Firms 1990	# Firms 1991	# Firms 1992	# Firms 1993	# Firms 1994	# Firms 1995	# Firms 1996	# Firms 1997	# Firms 1998	# Firms 1999	# Firms 2000
21 Heating, Ventilation, Refrigeration	Medium-low	1285	10.20	85	106	111	105	124	138	145	140	130	110	91
22 Computers and Peripherals	High	1713	13.60	91	110	122	120	174	191	199	215	193	162	136
23 Telecommunications	High	2294	18.22	100	144	160	193	224	276	275	273	231	197	221
24 Semiconductors and Electronics	High	32	0.25	3	3	3	2	3	3	3	4	3	3	2
25 Measurement and Control Equipment	High	714	5.67	44	52	64	68	75	74	76	75	63	64	59
26 Electrical Appliances and Components	High	660	5.24	38	37	48	61	69	73	74	74	71	65	50
27 Motor Vehicles and Parts	Medium-high	408	3.24	29	32	36	32	40	35	38	45	47	38	36
28 Aerospace and Parts	High	98	0.78	4	6	6	8	6	10	13	12	13	10	10
29 Other Transport	Medium-low	133	1.06	10	7	10	7	12	15	14	14	16	13	15
30 Misc. Manufacturing; and Other	Low	321	2.55	17	22	25	26	30	35	39	36	35	32	24
Total		12594	100.00	718	882	985	1069	1246	1359	1399	1433	1311	1146	1046

The 30 technology areas in Table 2 are the classifications that form the basis of the CHI Research patent and science paper database. CHI's technology area classification is a simplified version of the International Patent Classification (IPC) System developed by the World Intellectual Property Organisation (WIPO), and used by patent examiners to classify patents into the technology area that the patent advances. The CHI Research database is structured around 26 industries with the 30 technology areas identified *within* each industry. The 26 industries are: Aerospace; Automotive; Biotechnology; Chemicals; Computers; Conglomerates; Consumer Products; Electrical; Electronics; Energy; Engineering; Oil Field Services; Food, Beverage and Tobacco; Forest and Paper Products; Government Agencies; Health Care; Instrument and Optical; Machinery; Materials; Metals; Miscellaneous Companies; Pharmaceuticals; Research Institutes; Semiconductors; Telecommunications; Textiles; Universities. There are no sample firms in the Miscellaneous Machinery (technology area number 20).

Table 2 reports the sample firm-years classified according to the technology (within their industry) dominating their operations. The sample companies were classified first to a CHI industry (one of 26) and then to a CHI technology area within their industry (one of 30). See the Appendix for a full description of this process. For brevity, the technology area firm-years are reported in Table 2 aggregated over the 26 CHI industries.

of patent citation and scientific paper citation data, CHI Research. The CHI data is compiled from patents granted by the United States Patent and Trademark Organization (USPTO) to United States and non-United States applicants.¹⁵

This database, from which our dataset is compiled, covers about 67 percent of all U.S. patents (i.e., granted to U.S. and non-U.S. entities). The raw CHI database has annual firm-level data including counts of patents, citations in current patents referring back to the past five years of patents, and counts of scientific papers.

Patents granted by the USPTO usually contain eight to nine “References Cited—U.S. patents” on the front page of the document, as well as two references cited to foreign patents, and one or two references to non-patent references (research papers). These references link this patent to the earlier cited prior art (patents) and limit the claims of this patent. For a full description of the data and the development of patent-based measures for firm-level technological innovation, see Narin (1999).

To test our hypotheses, we aggregate the firm-level data from CHI into the “technology area” level of measurement. CHI has structured the raw data by assigning the patenting entities, first, to one of 26 industry classes and, second, to one of 30 technology areas within each of these industries.¹⁶ This structure yields 780 (26×30) possible technology areas. These 780 technology areas are the separate technology areas for which we aggregate the entity-level data to compute our measures of technology conditions. Entity-level patent, citation, and bibliographic data capture constructs that are distinct from the constructs captured by our aggregate technology area measures. In particular, the entity-level data captures the innovation activities of individual entities and research institutions. There is a large amount of variation in the value of these innovations and many more valueless innovations than economically successful innovations (e.g., Griliches 1990; Narin 1999). In contrast, aggregating the entity-level data to the technology area level (i.e., patent counts, citation, and science bibliographic data), provides measures of the states prevailing in the technology areas. Specifically, aggregation provides measures of the following:

- The cumulated history of all past investments in the technology area;
- The success of the past investments as given by the proliferation of patents in the technology area;
- The significance of the knowledge generated in the technology areas as reflected in the rate of citation of current patents to past patents in the technology area;
- The extent the technology area is advancing due to science rather than technological advances; and
- The rate that a technology area is developing new knowledge as given by the time taken to progress from past patents to current related patents.

Our variables computed from the CHI database are as follows:

Success rate (SR) is the “Number of Patents” in a technology area times “Current Impact Index” for the technology area. We multiply the patent count times the Current Impact Index (CII) because we need a weighting for the differences in the

¹⁵ The International Patent Classification (IPC) system is used by CHI because it has an industrial orientation. The U.S. patent classifications are based on invention, or art, rather than on application; for example, a classification describing a blade for a rotating bladed member might include both a desk fan blade and a jet engine propeller.

¹⁶ Tech-Line® industries are: Aerospace; Automotive; Biotechnology; Chemicals; Computers; Conglomerates; Consumer Products; Electrical; Electronics; Energy; Engineering, Oil Field Services; Food, Beverage, and Tobacco; Forest and Paper Products.; Government. Agencies; Health Care; Instrument. and Optical; Machinery; Materials; Metals; Misc. Companies; Pharmaceuticals; Research Institutes; Semiconductors; Telecommunications; Textiles; Universities.

strength of the technologies' underlying knowledge bases. Weighting is necessary because the number of patents on its own is a poor measure of the technology's importance (e.g., Narin 1995). The CII is measured as the number of citations from the current year's patents to patents issued in the most recent five years for the technology area, relative to the entire U.S. patent database.¹⁷ This measures the frequency with which patents previously issued in a technology area are cited on the front page of the patents granted in the current year in that technology area, compared to the average citation frequency in the entire U.S. patent database.¹⁸ Higher values of the success rate variable, *SR*, indicate a dynamic technology area in which significant new knowledge and valuable investment opportunities are being created.

Technology Complexity (TC) is the average number of scientific papers referenced on the front page of the patents in a technology area. Only papers published in highly ranked scientific journals are included in the counts of scientific papers. A higher number of citations to scientific research papers indicates that a technology area is advancing based on scientific research rather than on technological advances (Narin 1999). Technologies advancing on a science basis have higher imitation costs and "knowledge" barriers to entry, and are subject to reward systems that deal more efficiently with appropriation problems. Hence, higher values of *TC* indicate technology areas in which investing firms face lower appropriability problems.

Technology Development Period (TDP) is the median age in years of the prior, related patents cited on the front page of the patents in a technology area. This measure reflects the average number of years it takes to develop technological innovations in a technology area. Smaller *TDP* values reflect technology areas that are developing innovations at a relatively faster rate while larger values reflect technology areas that are slower moving or stagnant.

To facilitate our hypothesis tests, we manually coded each sample firm-year with the computed variables—success rate, technology complexity, and technology development period—for the technology area that dominates the firm's operations for that year. Where the firm has several areas of operation, the area with the highest sales was nominated as dominant. The Appendix provides details of the manual coding procedure. Summary statistics are provided for the accounting and technology variables in Table 3.

The accounting characteristics (*MV*, *BV*, and *SALES*) reported in Table 3 indicate that the sample includes a wide range of firm sizes. For example, the maximum firm sales are \$179,000 million, while the smallest is only \$1.28 million. Market value, book value, and earnings are also widely disbursed. Given our scaling procedure (division by sales), we do not expect that variation in firm size will drive our results.

The success rate (*SR*) variable varies between 15,392 and 0, indicating high investment opportunities in some technology areas and very little in others. Similarly, the range of the technology complexity (*TC*) variable is great: from 0 to 27.35 citations to scientific publications, which indicates that scientific research ranges between no direct influences in

¹⁷ In Narin (1999, 10–11), "When a U.S. patent is issued it has to satisfy three general criteria: it must be useful, it must be novel, and it must not be obvious. The novelty requirement is the primary factor leading to the references that appear on the front page of the patent. It is the responsibility of the patent applicant, his attorney, and the patent examiner, to identify, through references cited, all the important prior art upon which the issued patent improves."

¹⁸ Benchmarking against the total patent database means a CII value of 1.0 is an average citation frequency; a value of 2.0 is twice the average citation frequency; and a value of 0.25 is 25 percent of average citation frequency.

TABLE 3
Summary Statistics for the Pooled Sample for the 1990–2000 Period

Descriptive Statistics

	<i>MV</i>	<i>BV_{beg}</i>	<i>Earn</i>	<i>Sales</i>	<i>SR</i>	<i>TC</i>	<i>TDP</i>
Mean	3,400,000,000	822,000,000	144,000,000	2,270,000,000	1,795	1.64	8.10
Median	192,000,000	66,443,000	9,546,000	169,000,000	968	0.85	7.03
Maximum	511,000,000,000	78,900,000,000	13,500,000,000	179,000,000,000	15,392	27.35	39.24
Minimum	48,000	43,000	1,000	1,284,000	0.00	0.00	0.00
Std. Dev.	16,200,000,000	3,220,000,000	592,000,000	9,540,000,000	2,501	3.64	3.35
Skewness	12.96	9.22	9.41	9.72	2.64	4.77	1.77
Kurtosis	256.70	121.30	125.47	127.06	11.30	27.35	9.83

MV_{it} = market value of equity for firm *j* three months after fiscal year-end *t*;

BV_{it-1} = beginning book value of equity for firm *j* at the year-end *t*;

Earn_{it} = earnings before extraordinary items for year *t* for firm *j*;

Sales_{it} = sales for firm *j* for the year *t*;

SR_{it} = (aggregate technology area level measure of the) historical success rate of technological investments for firm *j* at time *t*;

TC_{it} = (aggregate technology area level measure of the) science linkage measure of appropriability for firm *j* at time *t*; and

TDP_{it} = (aggregate technology area level measure of the) technology development time indicator of the appropriability conditions for firm *j* at time *t*.

the technology area, to a high level of influence in others. Finally, the average technology development period (*TDP*) ranges from 0 (very fast innovation) to 39.24 years (slow innovation), with an average of 8.1 years. Long development periods suggest either inactive areas or areas in which technological advances take a long time, e.g., the development of pharmaceutical molecules. Table 4 reports the correlations among the variables. The Spearman rhos are shown above the diagonal, and the Pearson coefficients are shown below the diagonal.

Consistent with previously documented results, equity market value is positively and significantly associated with book value, earnings, and sales. Market value also has a significant, positive (negative) univariate relation with success rate (*SR*) and complexity (*TC*) (technology development period [*TDP*]). The technology development variables are also significantly correlated with each other, which is not surprising given that they are three proxies for the level of technology innovation in a specific technology area. The differences between the parametric and nonparametric correlations show that the *SR*, *TC*, and *TDP* inter-relationships are nonlinear (i.e., not interpretable using parametric correlation statistics). Further to this point, unreported plots of the relations between *SR*, *TC*, and *TDP* confirm that these three variables are negatively and nonlinearly related. Because the variables are negatively related to each other, they are not interactive in the sense that high levels of one variable do not accompany high levels of another variable.

Equity and financial risk measures, the standard deviation of stock returns (σRET) and the debt-to-equity ratio (*DE*), are negatively related to success rate (*SR*) and complexity (*TC*) and positively related to the technology development period (*TDP*). Hence, consistent with the theory framework, equity and financial risk are higher for firms facing lower technological success rate, lower complexity conditions, and longer technological development periods. Further, the market-to-book proxy for growth and the proxy for operating risk, the standard deviation of return on assets, are positively related to *SR* and *TC* and

TABLE 4
Summary Statistics for the Pooled Sample for the 1990–2000 Period

Correlation Matrix		Spearman's rho													
		MV	BV _{beg}	Earn	Sales	MV/sales	BV _{beg} /sales	Earn/sales	SR	TC	TDP	σRET	MB	DE	σROA
MV															
BV _{beg}	0.70**														
Earn	0.82**	0.76**													
Sales	0.61**	0.83**	0.76**												
MV/sales	0.14**	0.02*	0.04**	-0.03**											
BV _{beg} /sales	0.01	0.02*	-0.02*	-0.07**	0.35**										
Earn/sales	0.10**	0.01	0.12**	-0.05**	0.57**	0.39**									
SR	0.14**	0.10**	0.10**	0.07**	0.16**	0.05**	0.08**								
TC	0.05**	-0.02*	0.02*	-0.03**	0.27**	0.14**	0.15**	-0.09**							
TDP	-0.06**	-0.05**	-0.04**	-0.03**	-0.20**	-0.08**	-0.13**	-0.43**	-0.15**						
σRET	0.05**	0.09**	0.08**	0.08**	-0.17**	-0.05**	-0.13**	-0.23**	-0.08**	0.02*					
MB	0.20**	0.03**	0.12**	0.04**	0.48**	-0.08**	0.22**	0.12**	0.13**	-0.11**	-0.10**				
DE	-0.01	0.07**	0.04**	0.14**	-0.24**	-0.13**	-0.22**	-0.10**	-0.08**	0.17**	0.08**	-0.17**			
σROA	-0.03**	-0.04**	-0.04**	-0.06**	0.11**	0.00	0.06**	0.03**	0.13**	-0.02**	-0.07**	0.07**	-0.03**		

Pearson Correlations

*, ** Indicate significance at the .05 and .01 levels, respectively.
The Spearman's rho statistics are above the diagonal and the Pearson Correlations are below the diagonal.
MV_{*t*} = market value of equity for firm *j* three months after fiscal year-end *t*;
BV_{*t-1*} = beginning book value of equity for firm *j* at the year-end *t*;
Earn_{*t*} = earnings before extraordinary items for year *t* for firm *j*;
Sales_{*t*} = sales for firm *j* for the year *t*;
SR_{*t*} = (aggregate technology area level measure of the) historical success rate of technological investments for firm *j* at time *t*;
TC_{*t*} = (aggregate technology area level measure of the) science linkage measure of appropriability for firm *j* at time *t*;
TDP_{*t*} = (aggregate technology area level measure of the) technology development time indicator of the appropriability conditions for firm *j* at time *t*;
σRET_{*t*} = standard deviation of stock returns computed as a rolling measure over the time period the firm is included in the sample;
RET_{*t*} = stock returns including dividends ($P_t - P_{t-1} + D/P_{t-1}$);
MB_{*t*} = market value of equity divided by the book value of equity;
DE_{*t*} = total liabilities divided by the market value of equity; and
σROA_{*t*} = standard deviation of return on total assets.

negatively related to *TDP*. Thus, also consistent with the theory framework, expected growth is greater when the firm faces greater technological success rate and complexity conditions and shorter technology development period conditions. However, higher success rate and complexity, and shorter development period, conditions are associated with higher variability in the return on assets, because firms are in different lifecycle stages and not all individual firms and investments are successful.

Empirical Model

To examine the relevance of accounting information for firm value, we employ the following specification:

$$MV_{j,t} = \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}E_{j,t} + \alpha_t \quad (1)$$

where:

$MV_{j,t}$ = market value of equity for firm j three months after fiscal year-end t ;
 $BV_{j,t-1}$ = book value of equity for firm j at the year-end $t-1$ (i.e., beginning of period book value); and
 $E_{j,t}$ = earnings before extraordinary items in year t for firm j .

To test our hypotheses, we estimate the following equation using ordinary least squares.

$$MV_{j,t} = \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}Earn_{j,t} + \alpha_{3,t}(Earn_{j,t} * SR_{j,t}) + \alpha_{4,t}(Earn_{j,t} * TC_{j,t}) \\ + \alpha_{5,t}(Earn_{j,t} * TDP_{j,t}) + \alpha_{6,t}SR_{j,t} + \alpha_{7,t}TC_{j,t} + \alpha_{8,t}TDP_{j,t} + \pi_t \quad (2)$$

where:

$SR_{j,t}$ = success rate;
 $TC_{j,t}$ = technology complexity; and
 $TDP_{j,t}$ = technology development period.

Our theory suggests that the marginal investor forecasts the future earnings and value implications of current earnings in the context of the technological information. The coefficients on the interaction terms provide the test statistics for the hypothesized relations between the market value of equity and the interaction of technology conditions and earnings.

We estimate Equation (2) using market and accounting variables scaled by sales.¹⁹ Kothari et al. (2004) show that survival and data-trimming biases can be induced by data truncation and winsorizing procedures. We deal with the survival bias by employing an unbalanced panel for our sample; i.e., firms do not need to be listed for the full 11 years to be included. Outliers are identified from the regression residual diagnostics, and they are deleted if they are more than three standard deviations from the mean or have undue leverage.

¹⁹ As in Demers and Lev (2001) and Francis and Schipper (1999), among others.

IV. RESULTS

Primary Results

In Table 5, we report the estimated parameters from Equation (2) for the full sample (pooled results include unreported industry and year fixed effects) and subsamples for the individual years 1990–2000 (including unreported industry fixed effects).

Table 5 reports adjusted $\bar{R}^2$ s for the estimated regressions, all of which are material ranging from 0.38 to 0.58. Consistent with the established results in the accounting literature, Table 5 shows, for the pooled sample, that the estimated coefficients for book value (α_1) and earnings (α_2) are positive and significant for all the models tested. The estimated coefficients for both book value (α_1) and earnings (α_2) also have the expected positive signs for all individual years, and are significant for all years for book value and nine of the 11 years for earnings (insignificant in 1999, 2000).

The results for the pooled regressions, in the first five columns of results in Table 5, are consistent with the three hypotheses. Specifically, the estimated coefficient for the interaction term of earnings and success rate (α_3) is significant and positive (column 1); the coefficient for the interaction term of earnings and technology complexity (α_4) is significant and positive (column 2); and the interaction term of earnings and technology development period coefficient (α_5) is negative and significant (column 3). However, the coefficients for the technological variables (*SR*, *TC*, *TDP*) on their own are not significant in these regressions. In Table 5, column 4, we report the results from regressions including the interaction terms only (without the individual technological innovation variables), while column 5 reports the estimates from regressions including all the interaction terms and all the individual technological innovation variables. The estimated coefficients of the interaction terms are significant with the predicted signs. However, the technological variables on their own (*SR*, *TC*, *TDP*) are not significant consistent with our predictions. Table 5 also reports Akaike information criterion statistics, which are model selection criterion for non-nested models.²⁰ The smallest value indicates the preferred equation. In Table 5, the Akaike statistic is smallest for the pooled regression result, in column 4, that includes the interaction terms and excludes the technology variable intercepts.

Hence, the pooled results are consistent with H1 and H2, which predict a positive relation between the current market value of equity and the interaction between success rate and current earnings, and between technology complexity and current earnings, respectively. The primary results are also consistent with H3, which predicts a negative association between the market value of equity and the interaction between technology development period and earnings.

In Table 5, we also report the results for the individual years 1990–2000. These results are qualitatively consistent with the pooled results. The estimated coefficients for the interaction terms of earnings and success rate (α_3) are positive as predicted in all but one year and significant for five of the 11 years. The estimated coefficients for technology complexity (α_4) are positive in all years as predicted and significant for ten of the 11 years. The estimated technology development period coefficients (α_5) are negative as predicted in eight years and significant for four of the 11 years.

The theory framework predicts a conditional relation between the market value of equity and the technological innovation conditions via interactions with current earnings. To further evaluate this prediction we provide Wald tests (maximum likelihood estimates) in the last row of Table 5. We test the joint restrictions that the coefficients on the three

²⁰ Akaike information criterion is $[(-2(\text{Log. } L/t) + 2(k/t))]$, where $\text{Log. } L$ is a log likelihood function with k parameters estimated using t observations.

TABLE 5

Primary Hypothesis Tests

The Association of Market Value of Equity with the Interaction of Industry-Technology Area Technological Innovation Conditions with the Firm's Earnings
(firm-years with positive earnings and equity book value)

$$MV_{j,t} = \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}Earn_{j,t} + \alpha_{3,t}(Earn_{j,t} * SR_{j,t}) + \alpha_{4,t}(Earn_{j,t} * TC_{j,t}) + \alpha_{5,t}(Earn_{j,t} * TDP_{j,t}) + \alpha_{6,t}SR_{j,t} + \alpha_{7,t}TC_{j,t} + \alpha_{8,t}TDP_{j,t} + \pi_{j,t} \quad (2)$$

The hypothesis tests are as follows:

H₁: $\alpha_{3,t} > 0$;

H₂: $\alpha_{4,t} > 0$;

H₃: $\alpha_{5,t} < 0$.

		Pooled n = 12437		1990 n = 697	1991 n = 875	1992 n = 983	1993 n = 1058	1994 n = 1242	1995 n = 1355	1996 n = 1382	1997 n = 1407	1998 n = 1272	1999 n = 1139	2000 n = 1027
<i>BV_{log}</i>	(+)	0.861 7.42**	0.801 7.88**	0.886 8.06**	0.857 9.02**	0.228 2.07*	0.459 2.19*	1.121 4.97**	1.071 6.03**	0.772 4.30**	0.860 3.35**	0.750 4.18**	0.742 6.01**	0.848 8.10**
<i>Earn</i>	(+)	14.552 13.57**	13.424 16.60**	25.221 12.93**	15.167 13.52**	14.149 8.77**	19.805 3.54**	10.358 3.29**	12.345 3.98**	19.776 5.55**	8.646 2.44**	11.212 5.78**	23.588 5.36**	17.069 5.51**
<i>Earn</i> * <i>SR</i>	(+)	0.001 2.64**	0.001 5.51**	0.001 3.61**	0.001 3.61**	0.001 0.37	1.8e-04 0.08	0.003 2.02*	4.0e-04 0.52	2.5e-06 0.01	0.002 2.04*	0.001 2.19*	2.6e-04 0.44	-3.9e-04 -1.68
<i>Earn</i> * <i>TC</i>	(+)	1.104 4.26**	1.145 7.20**	1.207 4.66**	1.935 4.26**	1.935 3.14**	3.873 2.26*	1.706 2.75**	0.411 2.15*	0.200 0.66	1.545 3.00**	1.220 4.63**	0.374 2.10*	1.195 6.97**
<i>Earn</i> * <i>TDP</i>	(-)	-1.275 -6.13**	-0.612 -6.22**	-0.480 -3.02**	-0.480 -3.02**	0.215 0.75	-1.312 -2.27*	-0.010 -0.04	-0.028 -0.09	-0.831 -3.01**	0.078 0.21	-0.107 -0.59	-0.855 -2.10*	-0.819 -3.16**

(continued on next page)

TABLE 5 (continued)

	Pooled n = 12437	1990 n = 697	1991 n = 875	1992 n = 983	1993 n = 1058	1994 n = 1242	1995 n = 1355	1996 n = 1382	1997 n = 1407	1998 n = 1272	1999 n = 1139	2000 n = 1027
SR	2.3E-05 1.65	1.4E-04 -2.40*	-1.2E-04 -1.16	-1.9E-04 -2.27*	2.1E-06 0.05	5.7E-05 0.99	3.3E-05 0.60	-1.7E-05 -0.69	4.6E-06 0.127	4.3E-058 2.89**	2.5E-05 0.53	-1.7E-05 -0.26
TC		-0.010 -0.48	0.022 -2.67**	0.006 0.11	0.052 1.97	0.051 1.82	0.031 0.71	-0.011 -0.46	0.024 1.28	-0.011 -0.55	-0.101 -1.59	-0.066 -1.16
TDP		-0.015 -1.62	-0.044 -2.92**	-0.034 -2.44*	-0.026 -1.59	0.010 0.60	-0.040 -1.74	-0.023 -1.66	0.009 0.36	0.0135 0.85	-0.130 -3.07**	-0.053 -1.05
Intercept	-0.362 -5.02**	-0.307 -4.17**	0.285 -2.31*	0.457 2.46*	0.250 1.39	-0.048 -0.22	0.598 2.31*	0.469 2.81**	0.090 0.32	0.043 0.23	1.787 2.43**	0.841 1.38
Adj. R ²	0.38	0.41	0.39	0.44	0.44	0.39	0.49	0.55	0.52	0.53	0.35	0.38
Alaika Criterion	4.04	3.99	4.03	3.94	3.94							
Wald Test F-Stat.												
$\alpha_5 = \alpha_4 = \alpha_3 = 0$		18.24**	5.41**	4.25**	1.84	5.30**	3.92**	8.58**	6.25**	26.81**	6.33**	6.98**
$\alpha_6 = \alpha_7 = \alpha_8 = 0$		1.37	1.29	2.65	2.87*	1.29	1.73	1.01	0.55	3.16*	6.27**	0.82

* ** Indicate significance at the 0.05 and 0.01 levels, respectively.

The standard errors are Newey-West adjusted. Pooled estimations include fixed effects for year and SIC industries. Significance tests for the coefficients are one-tailed tests.

The Wald test statistic is a maximum likelihood estimate of how close the unrestricted estimates are to satisfying the specified restrictions under the null hypothesis. A significant F-statistic indicates the null hypothesis is rejected. The Akaike info criterion is a $[-2(\text{Log. L}(t) + 2(k/t))]$ Log-L log likelihood function, with k parameters and t observations. The smallest criterion indicates the most informative model.

Accounting variables are scaled by $\text{Sales}_{j,t}$. $MV_{j,t}$ is the market value of equity for firm j three months after fiscal year-end t; $BV_{j,t-1}$ is the beginning book value of equity for firm j at the year-end t; and $\text{Earnings}_{j,t}$ is earnings before extraordinary items for year t for firm j.

The three interaction terms measure the interaction of earnings for firm j in year t with the three measures of the technology innovation conditions in the firm's industry-technology area: technology innovation success rate $SR_{j,t}$; $TC_{j,t}$ is the (aggregate technology area level measure of the) science linkage measure of appropriability for firm j at time t; and length of technology development period in years $TDP_{j,t}$.

interaction terms equal zero ($\alpha_3 = \alpha_4 = \alpha_5 = 0$) and the coefficients on the technology variables equal zero ($\alpha_6 = \alpha_7 = \alpha_8 = 0$). Overall, the Wald tests reject the null hypothesis that the three interaction terms are jointly zero (significant F-statistics for the pooled sample and ten of the 11 individual years) but cannot convincingly reject the null hypothesis that the three technology variables are jointly zero (insignificant F-statistics for the pooled sample and seven of the 11 individual years). The Wald tests provide further statistical support for the hypothesized relations between the three technology variable interactions and the market value of equity.

We evaluated the sensitivity of these findings with respect to various alternative scaling variables such as total assets, and for subsamples of high and low technology areas. Our findings are generally consistent with the reported results. In all tests, we find that the coefficients, α_4 ($Earn * TC$) and α_5 ($Earn * TDP$), are significant and have the same signs as in the reported regressions. However, the evidence is less robust for the interaction between earnings and success rate ($Earn * SR$); the coefficient (α_3) is positive and significant when the interaction term, $Earn * SR$, is included in the regression, but the coefficients (α_3) are not consistently significant when all three interaction terms are in the regressions. Taking year-to-year variation in the data into account, overall we find evidence consistent with our theory that the market value of equity relations with earnings is conditional on the technology conditions.

In summary, our primary test results suggest that the interactions between current earnings and the technology condition variables are consistently associated with current market values. Hence, we conclude that the technology variables capture information that is not reflected in current earnings. The evidence is consistent with (1) the H1 prediction that the interaction between success rate and earnings is positively associated with market value; (2) the H2 prediction that the interaction between technology complexity and earnings is positively associated with market value; and (3) the H3 prediction that the interaction between technology development period and earnings is negatively associated with market value reflecting an expectation of positive earnings within the shorter investment horizon.

Additional Tests

In this section, we evaluate the robustness of our hypothesis test results to alternative explanations including earnings interactions with industry SIC codes, other accounting information, and earnings growth and risk measures. We also test two of our theory's implications; first, that the technology conditions predict future earnings (and therefore value); and second, that the technology conditions are important for non-patenting firms.

To begin, the primary tests from Equation (2) are rerun including terms for the interaction of earnings and SIC codes. These additional interaction terms address the concern that our main results are due to omitted correlated industry membership variables.²¹ Untabulated results from these additional regressions show that the coefficients for book value (α_1) and earnings (α_2) are positive and significant for most of the annual and all the pooled results, consistent with the primary tests. The coefficients for the interaction terms of earnings and success rate (α_3) and earnings and technology complexity (α_4) are significant and positive for five out of the 11 annual regressions and ten out of the 11 annual regressions

²¹ The SIC industries include the following: SIC 1 Agriculture, Energy and Waste Services, SIC 8 Primary and Fabricated Metals, SIC 2 Chemicals, Petroleum and Coal Products, SIC 9 Mining and Construction, SIC 3 Communication, SIC 10 Non-durable, Building Material, and Merchandisers, SIC 4 Electronic and Electric Equipment, SIC 11 Rubber, Plastics, Stone, Clay and Glass, SIC 5 Food, Textiles and Apparel, SIC 12 Services, SIC 6 Instrument and Other Manufacturing, SIC 13 Transportation Equipment and Products, SIC 7 Industrial Machinery and Equipment, SIC 14 Wood and Paper Products, Furniture, Printing, Publishing.

respectively, as in the primary results. For the pooled regression, the coefficients of these variables are significant and positive, consistent with the primary results in Table 5. Similarly, the coefficient for the interaction terms of earnings and technology development period (α_5) is significant and negative for five out of the 11 individual year regressions and for the pooled regression. Hence, these results including SIC industry codes interacted with current earnings in the estimations, confirm that our primary results are driven by the technology conditions themselves, and not industry classifications.

We also evaluate the impact of alternative accounting information, research and development (*RD*) expenditures and sales, general, and administration (*SGA*) expenditures, on our primary results. These robustness tests are motivated by Amir and Lev's (1996) finding that *SGA* is valued by investors as investment for some firms; and Lev and Sougiannis' (1996) finding, along with many other studies, that *RD* is valued by investors as a depreciating asset. In Table 6 we report the results for regressions that include *RD* and *SGA*, deflated by sales for firm j at time t , as additional variables in the primary model (Equation (2)) (also including fixed year and industry effects).

The regression in the last column of Table 6 includes all the Equation (2) variables as well as the additional *RD* and *SGA* variables. Regression results reported in columns 1, 2, and 3 include book value and earnings, sales-deflated *RD* and *SGA*, and one each of the interaction terms for earnings and technology conditions.

These results confirm our main findings. Coefficients for the interaction terms for success rate (α_3) and technology complexity (α_4) are positive and significant in all regressions. Coefficients for the technology development period interactions with current earnings (α_5), are all negative and significant. The estimated coefficient on research and development expenditure (α_6) is also reliably significant and positive in all regressions, incrementally to the interaction terms, while the sales, general, and administration expense coefficient (α_7) is not significant. These additional tests in Table 6 suggest that the technology condition variables capture information not reflected in the accounting variables, *RD* and *SGA*, thereby confirming the primary results.

To further evaluate the robustness of the main results, the hypothesis tests are subjected to additional controls for risk and growth, as suggested by the work of Collins and Kothari (1989) and Kothari and Sloan (1992). These studies find stronger contemporaneous returns-earnings relations when risk, growth, and earnings persistence measures are included in the regression analyses.

We run an expanded form of Equation (2) including three proxies for risk: the standard deviation of returns (σRET) reflecting equity market risk, the debt-to-equity ratio (D/E) reflecting financial risk, and the standard deviation of returns on operating assets (σROA) capturing operating risk. We also approximate growth using the market value of equity divided by the book value of equity (MB). Results for the pooled sample are reported in Table 7. The final column includes all the variables simultaneously. The first four columns report the results with the additional variables included one by one.

In Table 7, the interactions between current earnings and both success rate (α_3) and technology complexity conditions (α_4) are positive and significant, except for the success rate interaction in column 6. This latter result is a function of the positive correlation with expected growth for both success rate and current earnings. That is, *MB* and the current earnings and success rate variables are positively correlated (Pearson's: *MB*-earnings 0.22 and *MB*-success rate 0.12; Spearman's rho: *MB*-earnings 0.42 and *MB*-success rate 0.13). Coefficients for the interaction between current earnings and the technology development period (α_5) are negative and significant as predicted.

TABLE 6
Additional Tests Including Terms for Research and Development and Sales, General and Administration Expenses
(pooled analyses for firms with positive earnings and equity book value)

$$MV_{j,t} = \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}Earn_{j,t} + \alpha_{3,t}(Earn_{j,t} * SR_{j,t}) + \alpha_{4,t}(Earn_{j,t} * TC_{j,t}) + \alpha_{5,t}(Earn_{j,t} * TDP_{j,t}) + \alpha_{6,t}SR_{j,t} + \alpha_{7,t}TC_{j,t} + \alpha_{8,t}TDP_{j,t} + \alpha_{9,t}RD_{j,t} + \alpha_{10,t}SGA_{j,t} + \pi_t \quad (3)$$

H₁: $\alpha_{3,t} > 0$;

H₂: $\alpha_{4,t} > 0$;

H₃: $\alpha_{5,t} < 0$.

			1	2	3	4
<i>BV_{beg}</i>	(+)	+	0.627 6.97**	0.616 7.07**	0.665 7.60**	0.692 8.43**
<i>Earn</i>	(+)	+	13.661 14.73**	13.197 17.25**	23.654 13.35**	13.759 8.82**
<i>Earn * SR</i>	(+)	+	0.001 3.09**			0.001 3.71**
<i>Earn * TC</i>	(+)	+		0.968 3.97**		1.071 5.38**
<i>Earn * TDP</i>	(-)	-			-1.131 -6.03**	-0.423 -2.76**
<i>SR</i>			-7.5e-06 -0.39			-4.7e-06 -0.24
<i>TC</i>				-0.026 -1.30		-0.026 -1.28
<i>TDP</i>					0.029 2.51*	0.008 0.90
<i>RD</i>		+	8.907 12.51**	7.811 13.83**	8.441 12.15**	6.854 12.11**
<i>SGA</i>		+	0.252 1.14	0.303 1.52	0.172 0.79	0.296 1.51
Intercept			-0.605 -8.58**	-0.529 -7.89**	-0.858 -6.38**	-0.466 -3.99**
Adj. R ²			0.43	0.45	0.44	0.47
Akaike Criterion			3.95	3.92	3.94	3.88

*, ** Indicate significance at the 0.05 and 0.01 levels, respectively.

The standard errors are Newey-West adjusted. Pooled estimations (n = 12429) include dummy variables for year and industry effects. Significance tests for the coefficients of the accounting variables, technology condition interactions with *Earn_{j,t}*, *RD_{j,t}*, and *SGA_{j,t}*, are one-tailed tests. All the remaining significance tests are two-tailed. The Akaike info criterion is a $[-2(\text{Log. L}/t) + 2(k/t)]$ Log.L log likelihood function with k parameters estimated using t observations. The smallest Akaike criterion indicates the most informative model.

Market and accounting variables scaled by *Sales_{j,t}*. *MV_{j,t}* is the market value of equity for firm j three months after fiscal year-end t; *BV_{j,t-1}* is beginning book value of equity for firm j at the year-end t; *Earn_{j,t}* is earnings before extraordinary items for year t for firm j; *Sales_{j,t}* is sales for firm j for year t; and *RD_{j,t}* is research and development expenditures for firm j for year t; *SGA_{j,t}* is sales general and administration expenses (excluding RD) for firm j for year t; *SR_{j,t}* is the (aggregate technology area level measure of the) historical success rate of technological investments for firm j at time t; *TC_{j,t}* is the (aggregate technology area level measure of the) science linkage measure of appropriability for firm j at time t; *TDP_{j,t}* is the (aggregate technology area level measure of the) technology development time indicator of appropriability conditions for firm j at time t.

TABLE 7
Additional Tests
Adding Risk Variables and a Growth Variable
(firms with positive earnings and equity book value)

$$\begin{aligned}
 MV_{j,t} = & \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}Earn_{j,t} + \alpha_{3,t}(Earn_{j,t} * SR_{j,t}) + \alpha_{4,t}(Earn_{j,t} * TC_{j,t}) \\
 & + \alpha_{5,t}(Earn_{j,t} * TDP_{j,t}) + \alpha_{6,t}SR_{j,t} + \alpha_{7,t}TC_{j,t} + \alpha_{8,t}TDP_{j,t} + \alpha_{9,t}(Earn_{j,t} * \sigma RET_{j,t}) \\
 & + \alpha_{10,t}(Earn_{j,t} * MB_{j,t}) + \alpha_{11,t}(Earn_{j,t} * DE_{j,t}) + \alpha_{12,t}(Earn_{j,t} * \sigma ROA_{j,t}) \\
 & + \alpha_{13,t}\sigma RET_{j,t} + \alpha_{14,t}MB_{j,t} + \alpha_{15,t}DE_{j,t} + \alpha_{16,t}\sigma ROA_{j,t} + \tau_t
 \end{aligned} \tag{4}$$

$H_1: \alpha_{3,t} > 0;$
 $H_2: \alpha_{4,t} > 0;$
 $H_3: \alpha_{5,t} < 0.$

		1	2	3	4	5	6
<i>BV_{beg}</i>	+	1.237 5.89**	1.582 13.26**	1.260 6.09**	1.251 5.86**	1.24 5.91**	1.585 13.16**
<i>Earn</i>	+	14.558 7.60**	2.256 1.05	14.144 8.32**	13.264 7.53**	13.652 8.01**	3.00 1.24
<i>Earn * SR</i>	+	0.001 2.80**	4.2e-04 1.90*	0.001 2.30**	0.001 2.84**	0.001 3.06**	3.6e-04 1.47
<i>Earn * TC</i>	+	0.897 5.68**	0.307 2.01*	0.848 5.47**	0.839 5.66**	0.907 5.78**	0.247 1.70*
<i>Earn * TDP</i>	-	-0.772 -4.79**	-0.492 -3.36**	-0.641 -4.01**	-0.737 -4.67**	-0.736 -4.68**	-0.487 -3.12**
<i>SR</i>		-8.1e-06 -0.40	9.0e-06 0.56	-1.3e-05 -0.63	-7.1e-06 -0.36	-1.0e-05 -0.52	1.1e-05 0.67
<i>TC</i>		-0.008 -0.55	0.028 1.91*	-0.005 -0.34	-0.004 -0.29	-0.009 -0.58	0.032 2.30*
<i>TDP</i>		0.025 2.87**	0.008 0.99	0.018 1.99*	0.024 2.74**	0.024 2.73**	0.008 0.86
<i>Earn * σRET</i>		-15.400 -0.85					-10.348 -0.52
<i>Earn * MB</i>			2.53 5.70**				2.484 5.47**
<i>Earn * DE</i>				-6.465 -3.25**			-2.392 -1.84*
<i>Earn * σROA</i>					4.899 1.33		3.076 1.05
<i>σRET</i>	-	-6.066 -4.28**	-8.870 -8.90**	-7.352 -6.93**	-7.325 -6.85**	-7.331 -6.84**	-8.00 -5.35**
<i>MB</i>	+	0.193 2.52**	0.036 0.78	0.191 2.54**	0.194 2.51**	0.193 2.52**	0.040 0.82
<i>DE</i>	-	-0.168 -3.52**	-0.202 -6.46**	0.093 2.12*	-0.165 -3.45**	-0.164 -3.41**	-0.110 -2.60**
<i>σROA</i>	-	0.258 0.81	0.460 2.12*	0.249 0.80	-0.049 -0.14	0.260 0.81	0.258 1.23
Intercept		-0.143 -0.57	0.648 3.33**	-0.102 -0.44	-0.049 -0.21	-0.066 -0.27	0.581 2.82**
Adj. R ²		0.56	0.66	0.56	0.56	0.56	0.66
Akaike Criterion		3.62	3.37	3.61	3.62	3.62	3.37

(continued on next page)

TABLE 7 (continued)

*, ** Indicate significance at the .05 and .01 levels, respectively.

Significance tests are one-tailed tests. Akaike info criterion is $[(-2(\text{Log. } L/t) + 2(k/t))]$ with Log.L log likelihood function with k parameters estimated using t observations. The smallest Akaike criterion indicates the most informative model.

Market and accounting variables are scaled by sales. Estimated parameters are from pooled ordinary least square regressions. n is approximately 9079 after computing the additional variables. All regressions include unreported fixed effects for year and industry. Standard errors are Newey-West adjusted to minimize heteroscedasticity and serial correlation.

Additional variables include: $\sigma RET_{i,t}$, is the standard deviation of stock returns computed as a rolling measure over the time period the firm is included in the sample; $RET_{i,t}$, is stock returns including dividends $(P_t - P_{t-1} + D/P_{t-1})$; $MB_{i,t}$, is the market value of equity divided by the book value of equity; $DE_{i,t}$, is total liabilities divided by the market value of equity; $\sigma ROA_{i,t}$, is the standard deviation of return on total assets.

The results for the risk and growth measures are also consistent with Collins and Kothari (1989) and Kothari and Sloan (1992) in the following respects. The coefficients for the equity risk measure, α_{13} (σRET), and the financial risk measure, α_{15} (DE), are negative and significant, except for the DE coefficient in column 3. The estimated coefficients for the growth variable, α_{14} (MB), are positive and significant until the interaction term is included in the regressions, $Earn * MB$, at which time α_{14} (MB) becomes insignificant and the coefficient for the interaction term, α_{10} ($Earn * MB$), is positive and significant. This result suggests the growth (MB) relation with the market value of equity is through earnings.²² Looking at the conditional relations reflected in the interactions terms for equity and financial risk, ($Earn * \sigma RET$) and ($Earn * DE$), equity risk has a direct association with the market value of equity and is not conditional on earnings. The financial risk association with the market value of equity is both direct and conditioned on earnings. Overall, these results confirm our primary findings that the interactions of earnings with the technology condition variables capture information that is not reflected in the risk and growth measures included in these tests.

Collins and Kothari (1989) also suggest that the low contemporaneous association between changes in market value and unexpected earnings is a function of sample variation in the persistence of earnings. However, as they discuss, there are inherent methodological difficulties in estimating time-series, persistence tests. Therefore, we provide some indirect evidence indicating that our main findings are not driven by omitted variables relating to earnings persistence.

Our test refers to Francis and Smith (2005) who re-examine the persistence of the accrual and cash flow components of income using a cross-sectional design. They find, consistent with prior literature, that accruals are less persistent than cash flows. However, this effect disappears when the definitions for the accruals and cash flows variables better match economic events with accounting income (revenue or expense) events. We employ their cross-sectional design. Our theory implies that technology conditions have implications for the firms' investment decisions and future earnings. Therefore, including the technology condition interactions with accruals and cash flows in the persistence tests should include additional information about investment and expected earnings, thereby alleviating the biases in the measurement of earnings, i.e., conservatism bias due to the immediate expensing of some investments. Hence, we propose that the lower persistence of accruals will be

²² A caveat on this statistical result is that market value appears on both sides of the regression.

alleviated. To test this hypothesis, we estimate Equation (5) regressing one-period-ahead earnings on accruals, operating cash flows, and technology variable interactions.

$$\begin{aligned} Earn_{j,t+1} = & \alpha_{0,t} + \alpha_{1,t}Accrual_{j,t} + \alpha_{2,t}OCF_{j,t} + \alpha_{3,t}(Accrual_{j,t} * SR_{j,t}) \\ & + \alpha_{4,t}(Accrual_{j,t} * TC_{j,t}) + \alpha_{5,t}(Accrual_{j,t} * TDP_{j,t}) \\ & + \alpha_{6,t}(OCF_{j,t} * SR_{j,t}) + \alpha_{7,t}(OCF_{j,t} * TC_{j,t}) + \alpha_{8,t}(OCF_{j,t} * TDP_{j,t}) \\ & + \alpha_{9,t}SR_{j,t} + \alpha_{10,t}TC_{j,t} + \alpha_{11,t}TDP_{j,t} + \mu_r \end{aligned} \quad (5)$$

Another implication of our theory is that the technology conditions predict earnings. To validate this prediction we estimate the standard earnings persistence specification in Equation (6), with the addition of the technology variables. The dependent variable in Equation (6) comprises from one- up to eight-year-ahead earnings per share (*EPS*) deflated by average total assets per share. If the prediction is descriptive, and our measures are sufficiently precise, then we expect to observe a positive (negative) relation between technology success rate and technology complexity (technology development period) and future earnings ($EPS_{j,t+1, t+8}$).²³

$$EPS_{j,t+1, t+8} = \alpha_{0,t} + \alpha_{1,t}EPS_{j,t} + \alpha_{2,t}SR_{j,t} + \alpha_{3,t}TC_{j,t} + \alpha_{4,t}TDP_{j,t} + \mu_r \quad (6)$$

The results for Equations (5) and (6) are reported in Table 8.

The results for Equation (5) in the first column of Table 8 confirm the findings of previous research that the relation between one-period-ahead earnings and current cash flows is stronger than that between one-period-ahead earnings and current accruals. The Wald test indicates that the difference between the accrual and cash flow coefficients is statistically significant. The regression in the second column includes terms for the interactions of accruals and operating cash flows with the technology variables. Wald tests indicate that the estimated coefficients are not significantly different between the accrual and operating cash flows, or between the accruals interaction terms compared to the operating cash flow interaction terms. This evidence suggests (1) the technology condition variables capture investment and earnings growth information not reflected in the measures of accruals and cash flows; and (2) our main results are driven by the information in the technological innovation variables relating to the firm's investments and expected earnings.

The results for Equation (6), in the last eight columns of Table 8, are consistent with the proposition, implicit in our theory, that the technology conditions predict earnings. We find the *EPS* coefficients are positively associated with future earnings. Intuitively, the power of the regressions to explain future earnings, as indicated by the R^2 s, is decreasing over the estimation horizon (46 percent for one-year-ahead earnings, down to 11 percent for eight-years-ahead earnings). The estimated coefficients for success rate and technology complexity are significant and positive as predicted. Also, as proposed earlier in the "Technology Development Period" section, the technology development period is negatively associated with future earnings, reflecting the shorter prediction period, and the expectation of positive profits on average within the firm's investment horizon. There is a positive association between the success rate and future earnings, lasting for approximately three

²³ We utilize realized earnings as opposed to earnings forecasts as only a small proportion of our sample firms have analyst forecasts available.

TABLE 8

Additional Tests

Technological Innovation Impact on Differential Persistence of Accruals and Operating Cash Flows and Technological Innovation Capacity to Predict Future Earnings

$$Earm_{j,t+1} = \alpha_{0,j} + \alpha_{1,j}Accrual_{j,t} + \alpha_{2,j}OCF_{j,t} + \alpha_{3,j}(Accrual_{j,t} * SR_{j,t}) + \alpha_{4,j}(Accrual_{j,t} * TC_{j,t}) + \alpha_{5,j}(Accrual_{j,t} * TDP_{j,t}) + \alpha_{6,j}(OCF_{j,t} * SR_{j,t}) + \alpha_{7,j}(OCF_{j,t} * TC_{j,t}) + \alpha_{8,j}(OCF_{j,t} * TDP_{j,t}) + \alpha_{9,j}SR_{j,t} + \alpha_{10,j}TC_{j,t} + \alpha_{11,j}TDP_{j,t} + \mu_t \quad (5)$$

$$EPS_{j,t+1,t+8} = \alpha_{0,j} + \alpha_{1,j}EPS_{j,t} + \alpha_{2,j}SR_{j,t} + \alpha_{3,j}TC_{j,t} + \alpha_{4,j}TDP_{j,t} + \mu_t \quad (6)$$

		$EPS_{j,t+1}$ (n = 10,500)	$EPS_{j,t+1}$ (n = 10,500)	$EPS_{j,t+2}$ (n = 8,724)	$EPS_{j,t+3}$ (n = 7,244)	$EPS_{j,t+4}$ (n = 5,908)	$EPS_{j,t+5}$ (n = 4,628)	$EPS_{j,t+6}$ (n = 3,504)	$EPS_{j,t+7}$ (n = 2,554)	$EPS_{j,t+8}$ (n = 1,774)
EPS	+		0.830 21.73**	0.857 11.65**	1.003 8.68**	1.142 7.29**	1.45 5.87**	1.948 4.62**	1.976 2.92**	1.969 2.24*
Accrual	+	0.899 23.25**	1.069 11.18**							
OCF	+	0.978 30.32**	1.124 15.46**							
Accrual * SR	+	-3.4e-06 -0.19								
Accrual * TC	+	-0.011 -1.38								
Accrual * TDP	+	-0.021 -2.80**								
OCF * SR	+	3.2e-06 0.30								
OCF * TC	+	-0.007 -1.14								
OCF * TDP	+	-0.022 -3.69**								

(continued on next page)

TABLE 8 (continued)

	$Earn_{i,t+1}$ (n = 10500)	$EPS_{i,t+1}$ (n = 10500)	$EPS_{i,t+2}$ (n = 8,724)	$EPS_{i,t+3}$ (n = 7,244)	$EPS_{i,t+4}$ (n = 5,908)	$EPS_{i,t+5}$ (n = 4,628)	$EPS_{i,t+6}$ (n = 3,504)	$EPS_{i,t+7}$ (n = 2,554)	$EPS_{i,t+8}$ (n = 1,774)
SR	1.4e-06 1.05	3.7e-06 3.47**	6.3e-06 3.02**	8.1e-06 2.19*	1.1e-05 1.81	1.6e-05 1.49	2.5e-05 1.42	2.9e-05 0.99	4.3e-05 0.95
TC	0.003 3.58**	0.002 4.39**	0.004 4.46**	0.006 3.83**	0.008 3.50**	0.012 3.07**	0.013 2.50**	0.016 2.19*	0.030 2.42**
TDP	-1.4e-04 -0.24	-0.002 -4.47**	-0.003 -4.56**	-0.004 -4.31**	-0.005 -3.90**	-0.007 -3.84**	-0.009 -3.61**	-0.015 -3.77**	-0.021 -3.43**
Intercept	0.010 2.90**	0.037 1.88	0.136 3.05**	0.180 1.68	0.191 1.53	0.039 0.522	-0.030 -0.32	-0.129 -1.01	-0.091 -0.68
Adj. R ²	0.49	0.46	0.33	0.26	0.23	0.20	0.19	0.16	0.11
Wald tests									
$\alpha_1 = \alpha_2 = 0$	$\alpha_1 - \alpha_2 = -0.079$ F-stat. 15.99**								
$\alpha_3 = \alpha_6 = 0$	$\alpha_3 - \alpha_6 = -6.5e-06$ F-stat. 0.55								
$\alpha_4 = \alpha_7 = 0$	$\alpha_4 - \alpha_7 = -0.010$ F-stat. 0.48								
$\alpha_5 = \alpha_8 = 0$	$\alpha_5 - \alpha_8 = 0.001$ F-stat. 0.06								

*, ** Indicate significance at the 0.05 and 0.01 levels, respectively.

All regressions include unreported fixed effects for years. Significance tests for experimental variables are one-tailed tests. The Wald test statistic is a maximum likelihood estimate of how close the unrestricted estimates are to satisfying the specified restrictions under the null hypothesis. A significant F-statistic indicates the null hypothesis is rejected.

$Earn_{i,t+1}$ is earnings before extraordinary items for year $t+1$ for firm j ; $Accrual_{i,t}$ is $Earn_{i,t}$ minus operating cash flows for firm j for year t ; $OCF_{i,t}$ is operating cash flows for firm j for year t ; $Accrual_{i,t}$ and operating cash flow ($OCF_{i,t}$) variables are scaled by average assets, which is a rolling measure of average assets over the time period the firm is in the sample; $EPS_{i,t}$ is net income per share for firm j for year t . The earnings per share variables in Equation (6) are deflated by average assets per share at time t to control for firm size effects. $SR_{i,t}$ is the (aggregate technology area level measure of the) historical success rate of technological investments for firm j at time t ; $TC_{i,t}$ is the (aggregate technology area level measure of the) science linkage measure of appropriability for firm j at time t ; $TDP_{i,t}$ is the (aggregate technology area level measure of the) technology development time indicator of the appropriability conditions for firm j at time t .

years, consistent with the using up of investment opportunities amid competition from rival firms. In contrast, the positive association between technology complexity and future earnings continues for the eight years suggesting the relation is not limited by competition, consistent with the difficulty of imitating complex knowledge.

Finally, an implication of our theory is that the technology area conditions are important for all firms' investments and operations not just the R&D intense firms that are the prolific patenters. If this prediction is valid, then we expect the hypothesized relations to hold for the subsample of firms that do not patent. To test this proposition, we conduct the hypothesis tests on the subsample of firms (10,575 of the total 12,594 firm-years) that are not on the CHI database of patenters. We estimate Equation (7) including the technology variable intercepts, and *RD* interactions with earnings to rule out a non-patent related *RD* effect (i.e., *RD* firms use a variety of non-patent protection mechanisms; Cohen et al 2000).

$$\begin{aligned}
 MV_{j,t} = & \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}Earn_{j,t} + \alpha_{3,t}(Earn_{j,t} * SR_{j,t}) + \alpha_{4,t}(Earn_{j,t} * TC_{j,t}) \\
 & + \alpha_{5,t}(Earn_{j,t} * TDP_{j,t}) + \alpha_{6,t}SR_{j,t} + \alpha_{7,t}TC_{j,t} + \alpha_{8,t}TDP_{j,t} + \alpha_{9,t}RD_{j,t} \\
 & + \alpha_{10,t}(RD_{j,t} * SR_{j,t}) + \alpha_{11,t}(RD_{j,t} * TC_{j,t}) + \alpha_{12,t}(RD_{j,t} * TDP_{j,t}) + \tau_r \quad (7)
 \end{aligned}$$

Table 9 reports the results for the pooled sample.

The results for Equation (7) are consistent with the three hypotheses and the results reported in Table 5. When the control variables enter the estimation, there is a reduction in the size of the t-statistics but no qualitative difference in the results. The technological innovation variables (*SR*, *TC*, *TDP*) are insignificant, suggesting the technological impact on value is through the interaction with current earnings. Coefficients for the technology variable interactions are significant even after the research and development expenditures are included in the regression, suggesting a differential information effect in addition to *RD*. This additional evidence suggests that the technology conditions are relevant to the earnings-generation processes for all firms not just those firms that patent.

In summary, we have provided additional tests to show that our technology condition measures capture information not reflected in a range of alternative information sources. Our technology condition interactions remain significant predictors when we include terms for research and development expenditures; sales, general, and administration expenses; the interactions of current earnings with SIC codes; and the interactions of current earnings with growth and risk variables. Further, our tests suggest the interaction of the three technology conditions with accruals and with cash flows alleviates the lower persistence of accruals documented in the literature. Finally, the additional tests are consistent with our theory that technology conditions are instrumental in the earnings-generation process and this result holds for firms that do not patent.

V. CONCLUSIONS

This study makes an important contribution to our understanding of the information sources relevant for estimating the returns on firm-level investment and the market valuation of equity. The literature has focused on firm-specific financial and nonfinancial information. In this paper, we examine the association between market value and a completely different information source, one that reflects an underlying economic process, technological innovation.

TABLE 9
Additional Tests
Subsample of Non-Patenting Firms
(pooled analysis for firms with positive earnings and equity book value)

$$MV_{j,t} = \alpha_{0,t} + \alpha_{1,t}BV_{j,t-1} + \alpha_{2,t}Earn_{j,t} + \alpha_{3,t}(Earn_{j,t} * SR_{j,t}) + \alpha_{4,t}(Earn_{j,t} * TC_{j,t}) \\ + \alpha_{5,t}(Earn_{j,t} * TDP_{j,t}) + \alpha_{6,t}SR_{j,t} + \alpha_{7,t}TC_{j,t} + \alpha_{8,t}TDP_{j,t} + \alpha_{9,t}RD_{j,t} \\ + \alpha_{10,t}(RD_{j,t} * SR_{j,t}) + \alpha_{11,t}(RD_{j,t} * TC_{j,t}) + \alpha_{12,t}(RD_{j,t} * TDP_{j,t}) + \tau_t \quad (7)$$

$H_1: \alpha_{3,t} > 0;$

$H_2: \alpha_{4,t} > 0;$

$H_3: \alpha_{5,t} < 0.$

		1	2	3
<i>BV_{beg}</i>	+	0.829 8.48**	0.831 8.54**	0.672 7.87**
<i>Earn</i>	+	13.271 7.99**	14.314 12.16**	13.017 8.18**
<i>Earn * SR</i>	+	0.001 3.19**	0.001 4.93**	0.001 3.38**
<i>Earn * TC</i>	+	1.267 4.59**	1.148 6.69**	1.086 4.52**
<i>Earn * TDP</i>	—	−0.430 −2.68**	−0.557 −5.45**	−0.385 −2.49**
<i>SR</i>		1.6e−05 0.75		2.1e−06 0.07
<i>TC</i>		−0.021 −0.91		−0.041 −1.64
<i>TDP</i>		−0.015 −1.59		0.004 0.35
<i>RD</i>				6.308 3.11**
<i>RD * SR</i>				−3.0e−05 −0.11
<i>RD * TC</i>				0.156 1.41
<i>RD * TDP</i>				−0.036 −0.13
Intercept		0.015 0.13	−0.113 −2.17*	−0.291 −2.36*
Adj. R ²		0.44	0.44	0.46
Akaike Criterion		3.94	3.94	3.89

*, ** Indicate significance at the 0.05 and 0.01 levels, respectively.

Non-patenting firms are the sample firm-years that are not included in the CHI patenting database ($n = 10,565$) (the number of sample firm-years that is included in the CHI patenting database is 1872). Market and accounting variables are scaled by sales. Estimated parameters are from pooled ordinary least square regressions. Regressions include unreported fixed effects for year and SIC industries. Standard errors are Newey-West adjusted. Significance tests for experimental variables are one-tailed tests. Akaike info criterion is $[(-2(\text{Log. L}/t) + 2(k/t))]$ with Log.L log likelihood function with k parameters estimated using t observations. The smallest Akaike criterion indicates the most informative model.

Our study is motivated by the theory and evidence that technology is important for the investment decisions of all firms, regardless of whether they patent. Technological innovation is a determinant of the firm's decisions about inventive activities and technology-adoption decisions relating to products and processes. It is not the exclusive domain of firms that patent. Instead the combined investment decisions of all these firms serves to spread the technology into wider use. We identify three technology conditions from the economics of innovation literature: success rate, technology complexity, and technology development period. Our theory links the individual firm to technology areas by means of the firm's investments and related earnings. We argue that the marginal investor understands the earnings implications of the technology area conditions to which the firm's investments are exposed. The interaction of earnings with aggregate technology area conditions therefore contains information about the value of investments and expected earnings growth over and above earnings alone, and this information is capitalized into the market value of the firm.

Our primary results are consistent with the hypotheses that the interaction of earnings with the three aggregate measures of technology conditions—success rate, technology complexity, and technology development period—captures information that is not reflected in the commonly used variables in the valuation literature. Further tests suggest the importance of the aggregate technology conditions is robust to the presence of alternative information sources, including industry classification; research and development expenditures; sales, general, and administration expenses; risk; growth; and earnings persistence. Finally, we find results consistent with our theory that aggregate technology conditions are instrumental in the earnings-generation process for all firms, not just firms that patent.

Our study extends the valuation literature beyond firm-specific, nonfinancial variables, suggesting constructs and their empirical proxies that capture expected returns on the firm's investment that are not explicitly reflected in accounting data.

APPENDIX

DEFINING TECHNOLOGICAL INNOVATION AREAS

The World Intellectual Property Organization (WIPO) developed a technology area classification, the International Patent Classification (IPC). The database employed in this study utilizes a simplified form of this classification. The patent documents examined under this classification "*contain practically everything that represents an advance in the knowledge of mankind in the field of technology*" (<http://www.wipo.int/classifications/ipc/en/brochure/brochure.html>). Therefore, when measures are created from the *patents aggregated at the technology area*, and these measures are computed relative to the entire patent database, they capture conditions prevailing in individual technology areas—including the technology opportunity conditions in terms of success rates from innovation, technology cycle time from one invention until the next generation invention, and the extent the technology area is advancing on the basis of science inventions relative to technical advances. The following is an extract from the WIPO site, illustrating the eight broad technology classes and the range of product and service applications to which the first of these, Human Necessities, relates.

The IPC is a hierarchical system in which the whole technology area is divided into the following subdivisions: sections, classes, subclasses, and almost 70,000 groups (of which approximately 10 percent are "main groups," and the rest "subgroups"). They are as follows:

A	Human Necessities
B	Performing Operations; Transporting
C	Chemistry; Metallurgy
D	Textiles; Paper
E	Fixed Constructions
F	Mechanical Engineering; Lighting; Heating; Weapons; Blasting
G	Physics
H	Electricity

Each class has a title and a symbol. For example, the subsection "Foodstuffs; Tobacco" has the following four classes:

A21	Baking; Equipment for making or processing doughs; Doughs for baking
A22	Butchering; Meat treatment; Processing poultry or fish
A23	Foods or foodstuffs; Their treatment, not covered by other classes
A24	Tobacco; Cigars; Cigarettes; Smokers' requisites

Each subclass has a title and a symbol. The title consists of one or more words and the symbol consists of the symbol of the relevant class followed by a capital letter of the Roman alphabet. For example, class A21 ("Baking; Equipment for making or processing doughs; Doughs for baking") is divided into three subclasses (B, C, D) as follows:

A21B	Bakers' ovens; Machines or equipment for baking
A21C	Machines or equipment for making or processing doughs; Handling baked articles made from dough
A21D	Treatment, e.g., preservation, of flour or dough for baking, e.g., by addition of materials; Baking; Bakery products; Preservation thereof

Each main group and subgroup has a title and a symbol. For a main group, the second number consists of two zeros. For example, subclass A21B ("Bakers' ovens; Machines or equipment for baking") has five main groups (1/00, 2/00, 3/00, 5/00, 7/00), the first two of which are the following:

A21B 1/00	Bakers' ovens
A21B 2/00	Baking apparatus employing high-frequency or infra-red heating

Main group A21B 1/00 ("Bakers' ovens") is divided into 19 subgroups, the first four of which are the following:

A21B 1/02	. characterised by the heating arrangements
A21B 1/04	. . Ovens heated by fire before baking only
A21B 1/06	. . Ovens heated by radiators
A21B 1/08	. . . by steam-heated radiators

Source: <http://www.wipo.int/classifications/ipc/en/brochure/brochure.html>

Coding of Sample Firms on the Technology Metrics

The sample firms were first classified into their CHI industry (one of 26) and then into their technology area (one of 30). Then a recent graduate from a finance Ph.D. program classified the sample companies into their CHI industry and dominant technology. To

evaluate the replicability of this task, a random sample was independently classified by one of the authors with substantially the same results.

In most cases, there was no ambiguity about which industry and technology area was relevant because the CHI industry and technology classifications follow the International Patent Classification (IPC), which is industrial in orientation, i.e., once the nature of the business(s) and the product lines are known, the classification is usually obvious. In the case of diversified companies, we looked at the business segment data in the annual report to determine the dominant industry and technology. If the sample company has patents, the firm will already have U.S. and IPC technology area classifications assigned by the patent examiner. Some sample companies have patents, but are not included on the CHI database, as inclusion in this database is restricted to companies that patent prolifically. The firms that are listed in CHI were identified on the United States Patent and Trademark Office (USPTO) database site and their patents were searched on that database to identify the appropriate technology area. The CHI/USPTO/IPC concordance was then consulted to obtain the appropriate CHI technology area.

Companies often hold patents in several technology areas, and it was sometimes necessary to select the dominating technology. Although the technologies in which a company has patents may be different, there is consistency in the level of the technology a firm invests in. So, for example, companies operating in higher (lower) technology areas tend to have higher (lower) levels of the technology condition metrics across the different areas in which they operate. See Table A1 for technology level classifications. Moreover, a company may be exposed to some of the same underlying knowledge base across several of the different technologies in which they invest. Accordingly, if there is a misclassification with respect to the CHI industry and technology classifications, then the alternate choices of technology area have similar technology level metrics (i.e., a higher (lower) technology has higher (lower) value of the patent counts, citations from prior patents, and so forth). The error would not have a material effect on the analysis. An actual example (identified in Table A1) is a company that was classified by the Ph.D. graduate into the "Biotechnology" industry and the "Pharmaceuticals" technology, and the author reversed the classifications in the random check. Table A1 shows that these would have roughly the same metrics if uncorrected. Another example is a company classified into the "Electronics" industry and "Semiconductors and Electronics" technology by the Ph.D. student and vice versa by the author. Thus we conclude that the classification can be replicated.

Finally, the data extract in Table A1 shows that the technology condition data varies over the sample period.

TABLE A1
Example of Time Variation in Patent Data for Industry-Technology Areas Employed to Measure Technology Area Conditions

Year	Industry	Technology Area (within the column 2 industry)	Cites per Patent	Current Impact Index	Tech. Strength	Tech. Cycle Time	Science Linkage
1990	Biotechnology	08: Pharmaceuticals	14.5	1.9	73.7	6.2	5.5
1991	Biotechnology	08: Pharmaceuticals	9.9	0.8	20.9	6.8	5.2
1992	Biotechnology	08: Pharmaceuticals	14.5	0.9	36.0	6.1	9.3
1993	Biotechnology	08: Pharmaceuticals	10.0	0.7	24.4	6.2	8.9
1994	Biotechnology	08: Pharmaceuticals	13.8	1.1	35.8	5.8	5.6
1995	Biotechnology	08: Pharmaceuticals	7.6	1.2	63.7	7.4	18.7
1996	Biotechnology	08: Pharmaceuticals	7.4	1.3	105.3	8.5	19.8
1997	Biotechnology	08: Pharmaceuticals	4.1	1.6	311.6	8.6	26.4
1998	Biotechnology	08: Pharmaceuticals	2.3	1.3	261.0	8.3	30.4
1999	Biotechnology	08: Pharmaceuticals	0.8	1.8	438.0	8.1	28.6
2000	Biotechnology	08: Pharmaceuticals	0.2	1.2	285.9	9.6	28.6
		Average 1990-2000	7.7	1.3	150.6	7.4	17.0
1990	Pharmaceuticals	09: Biotechnology	9.9	0.7	108.6	6.6	6.3
1991	Pharmaceuticals	09: Biotechnology	8.9	0.7	98.1	7.1	7.4
1992	Pharmaceuticals	09: Biotechnology	5.6	0.8	155.2	7.6	8.6
1993	Pharmaceuticals	09: Biotechnology	5.7	0.8	189.8	7.5	11.9
1994	Pharmaceuticals	09: Biotechnology	4.9	0.7	140.3	7.2	10.5
1995	Pharmaceuticals	09: Biotechnology	5.0	0.6	141.5	7.9	12.8
1996	Pharmaceuticals	09: Biotechnology	4.2	0.5	184.6	7.9	19.5
1997	Pharmaceuticals	09: Biotechnology	1.8	0.8	391.1	8.4	23.8
1998	Pharmaceuticals	09: Biotechnology	1.1	0.8	552.7	7.6	24.7
1999	Pharmaceuticals	09: Biotechnology	0.6	0.5	349.8	7.2	19.6
2000	Pharmaceuticals	09: Biotechnology	0.2	0.5	297.4	6.9	17.9
		Average 1990-2000	4.4	0.7	237.2	7.5	14.8

(continued on next page)

TABLE A1 (continued)

1990	Electronics	23: Semicon. and Electronics	8.4	1.2	1108.6	7.0	0.9
1991	Electronics	23: Semicon. and Electronics	8.6	1.3	1479.9	5.9	0.8
1992	Electronics	23: Semicon. and Electronics	8.0	1.2	1386.7	5.8	0.7
1993	Electronics	23: Semicon. and Electronics	7.9	1.2	1526.9	5.7	0.8
1994	Electronics	23: Semicon. and Electronics	7.5	1.2	1530.9	5.8	0.8
1995	Electronics	23: Semicon. and Electronics	6.9	1.2	1617.7	5.7	1.0
1996	Electronics	23: Semicon. and Electronics	5.9	1.1	1321.6	6.0	1.1
1997	Electronics	23: Semicon. and Electronics	4.7	1.2	1516.7	6.5	0.9
1998	Electronics	23: Semicon. and Electronics	3.3	1.1	2000.3	6.1	0.8
1999	Electronics	23: Semicon. and Electronics	2.0	1.3	2463.1	6.2	0.7
2000	Electronics	23: Semicon. and Electronics	0.7	1.3	2726.2	6.0	0.6
		Average 1990-2000	5.8	1.2	1698.1	6.1	0.8
1990	Semiconductors	23: Semicon. and Electronics	16.0	1.7	460.6	5.8	1.1
1991	Semiconductors	23: Semicon. and Electronics	15.2	1.9	748.6	5.8	0.8
1992	Semiconductors	23: Semicon. and Electronics	16.8	2.0	883.4	5.5	0.8
1993	Semiconductors	23: Semicon. and Electronics	16.5	1.9	1003.9	5.3	1.0
1994	Semiconductors	23: Semicon. and Electronics	14.5	1.8	1246.2	5.4	0.9
1995	Semiconductors	23: Semicon. and Electronics	12.3	2.0	1623.8	5.3	1.2
1996	Semiconductors	23: Semicon. and Electronics	11.2	1.8	1716.6	5.5	1.4
1997	Semiconductors	23: Semicon. and Electronics	9.0	2.1	2637.3	5.6	1.2
1998	Semiconductors	23: Semicon. and Electronics	5.9	1.9	3361.9	5.4	1.2
1999	Semiconductors	23: Semicon. and Electronics	3.4	2.5	6276.3	5.5	1.3
2000	Semiconductors	23: Semicon. and Electronics	1.3	2.6	8873.8	5.0	1.3
		Average 1990-2000	11.1	2.0	2621.1	5.5	1.1

Source: Data is from the commercial provider CHI Research.

TABLE A2
Classification of Manufacturing Industries Based on Technological Innovation Intensity

The OECD method employs data from national accounts and industry surveys to derive three indicators of technology intensity reflecting "technology-producer" and "technology-user" aspects: (1) R&D expenditures divided by value added; (2) R&D expenditures divided by production; and (3) R&D expenditures plus technology embodied in intermediate and investment goods divided by production. The classification employed in this paper is the 2003 OECD scoreboard computation using R&D expenditure and output data for 12 OECD countries for the period 1991–1999. In the absence of updated, input-output tables (required for estimating embodied technology), statisticians were only able to compute the first two indicators for 2003.

	ISIC Rev. 3	1999						1991					
		R&D Divided by Production			R&D Divided by Value Added			R&D Divided by Production			R&D Divided by Value Added		
		Aggregate Intensity	Median Intensity		Aggregate Intensity	Median Intensity		Aggregate Intensity	Median Intensity		Aggregate Intensity	Median Intensity	
High-technology industries													
Aircraft and spacecraft	353	10.3	10.4		29.1	27.5		13.9	12.9		34.7	32.1	
Pharmaceuticals	2423	10.5	10.1		22.3	25.8		9.4	8.7		20.6	19.7	
Office, accounting and computing machinery	30	7.2	4.6		25.8	15.1		10.9	6.4		29.4	15.2	
Radio, TV and communications equipment	32	7.4	7.6		17.9	22.4		7.9	8.2		17.0	21.5	
Medical, precision and optical instruments	33	9.7	5.6		24.6	11.9		6.6	6.1		15.6	12.5	
Medium-high-technology industries													
Electrical machinery and apparatus, n.e.c.	31	3.6	2.3		9.1	6.7		4.2	2.6		9.3	5.9	
Motor vehicles, trailers and semi-trailers	34	3.5	2.8		13.3	11.7		3.7	3.0		14.3	11.9	
Chemicals excluding pharmaceuticals	24 excl. 2423	2.9	2.2		8.3	7.1		3.4	2.8		9.8	8.0	
Railroad equipment and transport equipment, n.e.c.	352 + 359	3.1	2.8		8.7	7.9		2.9	2.1		7.6	5.4	
Machinery and equipment, n.e.c.	29	2.2	2.1		5.8	5.3		1.9	2.0		4.6	4.7	

(continued on next page)

TABLE A2 (continued)

Medium-low-technology industries		OECD countries											
		United States	Canada	Japan	Denmark	Finland	France	Germany	Ireland	Italy	Spain	Sweden	and the United Kingdom
Building and repairing of ships and boats	351	1.0	1.0	1.0	3.1	2.9	0.9	0.9	2.8	2.6	1.5	3.8	1.5
Rubber and plastics products	25	1.0	1.1	2.7	3.0	3.0	1.0	0.6	2.6	5.4	2.4	1.5	1.6
Coke, refined petroleum products and nuclear fuel	23	0.4	0.3	1.9	2.7	2.7	1.2	0.7	2.6	5.4	2.4	1.5	1.6
Other non-metallic mineral products	26	0.8	0.6	1.9	1.9	1.3	1.0	0.6	2.4	2.0	1.6	1.6	1.6
Basic metals and fabricated metal products	27–28	0.6	0.5	1.6	1.6	1.4	0.7	0.6	2.0	1.6	1.6	1.6	1.6
Low-technology industries													
Manufacturing, n.e.c.; Recycling	36–37	0.5	0.5	1.3	1.3	1.2	0.5	0.4	1.2	0.8	0.3	0.3	0.3
Wood, pulp, paper, paper products, printing and publishing	20–22	0.4	0.1	1.0	1.0	0.3	0.3	0.1	0.8	0.3	0.3	0.3	0.3
Food products, beverages and tobacco	15–16	0.3	0.3	1.1	1.1	1.0	0.3	0.3	1.1	1.1	1.1	1.1	1.1
Textiles, textile products, leather and footwear	17–19	0.3	0.4	0.8	0.8	1.0	0.2	0.3	0.7	0.7	0.7	0.7	0.7
Total manufacturing	15–37	2.6	2.2	7.2	7.2	6.5	2.5	2.0	7.0	7.0	5.7	5.7	5.7

Source: OECD Science, Technology and Industry Scorecard (2003)
Based on data for 12 OECD countries: United States, Canada, Japan, Denmark, Finland, France, Germany, Ireland, Italy, Spain, Sweden, and the United Kingdom.
Aggregate R&D intensities calculated after converting countries' R&D expenditures, value added, and production using GDP PPPs.

Source: OECD Science, Technology and Industry Scorecard (2003)

Sources: OECD Science, Technology and Industry Scoreboard (2007). Based on data for 12 OECD countries: United States, Canada, Japan, Denmark, Finland, France, Germany, Ireland, Italy, Spain, Sweden, and the United Kingdom. Aggregate R&D intensities calculated after converting countries' R&D expenditures, value added, and production using GDP PPPs.

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An Empirical Analysis of the Decline in the Information Content of Earnings Following Restatements

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ABSTRACT: Regulatory officials and market analysts have speculated that the loss of credibility in subsequently reported financial information is a long-lasting consequence of earnings restatements. I measure the information content of earnings using a standard earnings-returns framework over several years surrounding restatements to examine characteristics of the decline in the information content of earnings. Results indicate that although the information content of earnings declines following restatements, the loss is temporary. In particular, the earnings response coefficients for earnings announcements surrounding restatements exhibit a U-shaped pattern in which they are no longer significantly lower in the post-restatement period over an average of four quarters. The extent to which the earnings of restatement firms suffer a loss of information content varies across several dimensions. First, the duration of the loss is greater for firms that restate earnings to correct revenue recognition errors and for restatements that result in a large decline in the stock price at the announcement date. Second, there is not a loss in the information content of earnings for firms that make changes to their financial reporting governance structures following restatements. Overall, the evidence in this paper is consistent with a short-term decline in investor confidence regarding financial reporting following restatements, but shows that suspicion regarding the information loss of post-restatement earnings in the long-term is unwarranted.

Keywords: *earnings restatement; information content of earnings; investor confidence; corporate governance; earnings response coefficient.*

JEL Classification: G14, M41, M43, M45.

This paper is based on my dissertation at The University of North Carolina at Chapel Hill. I thank the members of my dissertation committee for their valuable guidance and support: Ed Maydew and Jana Smith Raedy (co-chairs), John Hand, Wayne Landsman, and Mark Lang. I appreciate the constructive comments of two anonymous referees and the suggestions of Dan Dhaliwal (editor). I also thank Leonce Barger, Robert Bushman, Zhonglan Dai, Hemang Desai, Brad Lindsey, Chris Petrovits, Kevin Raedy, Brian Rountree and workshop participants at Arizona State University, Florida State University, Southern Methodist University, The University of Arizona, University of Colorado, University of Connecticut, University of North Carolina, and University of Notre Dame for helpful suggestions. A previous version of this paper was titled "The Information Content of Earnings Following Restatements." Financial support from the Deloitte Foundation is gratefully acknowledged. Data used in this study are available from public sources identified in the text.

Editor's note: This paper was accepted by Dan Dhaliwal.

Submitted April 2007
Accepted September 2007

I. INTRODUCTION

This paper examines characteristics of the decline in the information content of earnings following restatements. Many restatements have been announced in the last decade, ranging from well-known incidents involving firms such as WorldCom, Qwest, and Tyco, to numerous, less noteworthy restatements. In response to the increase in earnings restatements, regulators have expressed concerns about the effect of the large number of restatements on investor confidence in our financial reporting system. The Chairman of the SEC cited the “rise in restatements and the broader damage to investor confidence” along with the ineffectiveness of existing regulatory measures as motivating factors for new regulations imposed by the Sarbanes-Oxley Act of 2002.¹ Additional concern regarding the effect of restatements is expressed in the General Accounting Office’s special report for the U.S. Senate on the rising incidence and market consequences of restatements (GAO 2002). The GAO report stated that “the growing number of restatements appears to have shaken investors’ confidence in our financial reporting system” (GAO 2002, 6). The GAO report also noted that a June 2002 USB/Gallup survey indicated that accounting errors are a serious problem that has negatively impacted securities markets.

Concern regarding earnings restatements is not limited to statements by SEC and other regulatory officials. Analysts have also demonstrated concern about the impact of restatements on the market’s assessment of the quality of reported financial information. In an article describing government involvement in enacting regulatory reforms following recent earnings restatement activity, a senior strategist from Morgan Stanley stated, “Investors don’t have confidence that the corporate numbers game will end soon. We need a restoration of trust from every entity” (Borrus 2002). With respect to restatements announced by individual firms, analysts have widely commented about the potentially damaging effect of restatements on the credibility of financial information. For example, following the announcement of the need to restate fiscal 1999 earnings by McKesson HBOC, Inc. in May 1999, an analyst for Merrill Lynch commented, “this clearly raises the question about whether any number can be trusted at HBOC” (Abate 1999).

Although extensive anecdotal evidence suggests concern about the quality of financial reporting of firms that have issued earnings restatements, empirical evidence regarding the information content of earnings following restatements is limited. Anderson and Yohn (2002) and Wu (2002) analyze the market’s response to earnings following restatements and find that the response to earnings is dampened in the short-term. While these papers provide evidence consistent with investors responding less to post-restatement earnings in the short-term, the literature thus far has not addressed the question of whether investors are wary in the long-term of information reported by firms that have issued inaccurate reports. SEC regulators and market analysts have speculated that restatements cause damage to the long-term credibility of information. However, whether restatements have a long-term impact on the information content of earnings remains an open question.

I measure the information content of earnings over multiple reporting periods surrounding restatements to address this issue. Using a standard earnings-returns framework, I measure the earnings response coefficient (ERC) over several years to examine whether restatements result in a long-term decline in the information content of earnings. Given that previously reported earnings have been restated to correct accounting errors, I hypothesize that investors believe subsequently reported earnings have lower information content due

¹ Remarks before the Economic Club of New York by Chairman Harvey L. Pitt, June 26, 2002.

to uncertainty regarding whether post-restatement earnings provide a high-quality indication of expected future cash flows.

I also examine cross-sectional variation in the duration of the decline in the information content of earnings across characteristics of restatements. Specifically, I test for differential impacts for firms that issue restatements caused by revenue recognition errors and for firms that experience a large stock price decline at the restatement announcement date. I also measure whether firm-level changes to the governance of financial reporting, namely dismissal of the firm's auditor or replacement of top management following restatements, are associated with differences in the length of the decrease in the information content of earnings.

Results indicate that the decline in the information content of earnings following restatements is temporary. The price response to earnings information exhibits a U-shaped pattern over time, where the ERC is significantly lower for several quarters but then increases substantially by the fourth quarter following the restatement. This pattern indicates that investors regain confidence in earnings news within a short period following restatement announcements, which suggests the market's concern regarding earnings reported following restatements appears to be transitory.

Tests based on subsamples of restatements indicate that the decrease in the information content of earnings is longer-lasting for firms that issue restatements to correct revenue recognition problems relative to other reasons for restatements, and for restatements that cause a large stock price decline at the restatement announcement. These results suggest that investors are more apprehensive regarding the quality of post-restatement information for these groups of firms. In addition, tests of the effect of firm-level actions, such as replacement of the firm's auditor or top management following a restatement, indicate that the information content of earnings is not lower in the post-restatement period for firms that make changes to their governance structures. This result is consistent with investors having greater confidence in reported financial information following a restatement when firms take actions demonstrative of a commitment to high-quality financial reporting.

This paper makes several contributions to the restatement literature. First, this study examines whether the decrease in the information content of earnings news is a lasting phenomenon, as suggested by SEC regulators and financial analysts in the wake of the large number of earnings restatements announced over the last decade. Though results are generally consistent with the idea that capital market participants are wary of information released following restatements, concern regarding reported earnings is not long-lasting. Second, examination of cross-sectional variation in the duration of the decline in the information content of earnings surrounding restatements extends our knowledge of how the consequences suffered by restatement firms differs along various dimensions, including the reason for restatement and firm-initiated responses following the restatement. Third, this paper provides evidence of an association between changes in corporate governance following restatements and the information content of earnings. This result complements a growing body of research that analyzes how firms improve monitoring mechanisms following accounting failures (e.g., Farber 2005; Srinivasan 2005; Desai et al. 2006). Taken collectively, the findings of this study are likely to be of interest to regulatory officials as the results provide insight into the way the market responds to earnings from firms that have admitted to reporting prior-period accounting earnings that were not prepared in compliance with GAAP.

The remainder of the paper is organized as follows: Hypothesis development is described in Section II and Section III outlines the research design and sample selection. Empirical results are presented in Section IV and conclusions follow in Section V.

II. HYPOTHESIS DEVELOPMENT

The number of firms issuing earnings restatements has increased dramatically since the mid-1990s (GAO 2002). The negative stock price response to restatement announcements is well documented in the literature, where the immediate cumulative average abnormal return for short windows surrounding the restatement announcement range from -9.0 percent (Hribar and Jenkins 2004) to -11.2 percent (Wu 2002).² Other short-term, firm-level consequences suffered immediately following restatements include diminished economic prospects, as measured by a downward revision in earnings forecasts, and increased uncertainty, as measured by an increase in analyst forecast dispersion (Palmrose et al. 2004). Restatements also lead to increases in a firm's cost of equity capital (Hribar and Jenkins 2004). In addition, outside directors suffer high turnover and labor market penalties (Srinivasan 2005), and the incidence of lawsuits filed against firms following the restatement is high (Palmrose and Scholz 2004).³

Anderson and Yohn (2002) and Wu (2002) provide initial empirical evidence regarding short-term changes in the information content of earnings following restatements. These papers find a decrease in the response coefficient for the earnings disclosure in the first two quarters (Wu 2002) or the first annual earnings release (Anderson and Yohn 2002) immediately following restatements. The short-term decrease in the information content of earnings is consistent with investor skepticism in the post-restatement period as to whether earnings information released by the firm will be credible signals of the firm's future cash flows.⁴ However, articles in the business press are replete with comments made by regulatory officials and financial analysts that suggest that market participants are apprehensive of reported earnings in the long-term following restatements (e.g., Borrus 2002). Following Waste Management's earnings restatement in 1999, for example, analysts commented that it would take time for the company to restore its credibility. One analyst stated, "I think they've really got a lot of work in front of them. It could take well over a year before they're back on track" (Goldberg 1999). Based on the concern expressed by regulators and analysts regarding the market's lack of confidence in long-term financial reporting following restatements, the first hypothesis, stated in alternative form, is as follows:

H1: There is a long-term decrease in the information content of earnings following earnings restatements.

² The GAO report cumulated short-term returns and found that, over the five-and-a-half-year period covered in their analysis, publicly traded companies that restated prior-period results cumulatively lost more than \$100 billion in market capitalization surrounding restatement announcements.

³ The authoritative literature that addresses earnings restatements includes SFAS No. 154 (FASB 2005) and SAB No. 108 (SEC 2006). SFAS No. 154 replaced APB No. 20 (APB 1971) when it was issued in May 2005, thus APB No. 20 was in effect during the sample time period. APB No. 20 defined a prior period error and stated that the correction of an error should be reported as a prior period adjustment. SFAS No. 154 does not modify accounting treatment of a prior period error, though the statement explicitly defines a restatement as the revising of previously issued financial statements to reflect the correction of an error, which is necessary when a restatement is material. SAB No. 108 specifies the appropriate method of evaluating materiality.

⁴ The sample in this study is derived from the U.S. Government Accounting Office (GAO) restatement report, which consists of restatements resulting from "aggressive" accounting practices, intentional and unintentional misuse of facts applied to financial statements, oversight and misuse of accounting rules, and fraud (GAO 2002, 76). Therefore, all the restatements in the sample are based on incidents where previously reported information was not prepared in accordance with GAAP and the post-restatement period is a setting in which uncertainty regarding the quality of information likely exists. Extant research has examined changes in the information content of earnings in different settings where information uncertainty likely exists, including proxy contests (Collins and DeAngelo 1990), large write-offs (Elliott and Hanna 1996) and differential GAAP regimes (Collins and Salatka 1993).

In addition to examining the extent of the decline in the information content of earnings, I examine whether the duration of the decline differs for certain groups of restatements. Though all restatements imply that previously released information was in error, the range of reasons for restatement varies substantially. Reasons for restating prior-period earnings range from the reversal of intentionally recorded false sales to correction of mathematical errors caused by defects in accounting software.

Restatements to correct revenue recognition errors have received considerable attention from regulatory officials (e.g., GAO 2002) and academic research (e.g., Palmrose et al. 2004). Corrections of revenue recognition errors involve either a change in timing of recognition or reversal of previously recorded revenues. In both cases, a firm's time-series of revenue is affected by the restatement. If revenue patterns over time (i.e., revenue persistence) are an important input in predicting future firm performance (Jegadeesh and Livnat 2006), then investors need information revealed by corrected revenue figures in the post-restatement period to reassess a firm's revenue persistence and to understand the implications for future cash flows. Empirical research (e.g., Ertimur et al. 2003) that finds evidence that market participants focus on revenues rather than the expense component of earnings (attributed to revenues having greater persistence for future earnings) also provides support for testing differential reactions in the information content of earnings for restatements caused by revenue recognition errors and other errors.

I hypothesize that the decline in the information content of earnings will be longer-lasting for restatements caused by revenue recognition errors relative to restatements made for other reasons. A differential reaction following correction of revenue recognition issues is consistent with the market's need for information from reporting periods following the restatement in order to reassess a firm's revenue persistence and make inferences regarding the information content of reported earnings. Therefore, the second hypothesis, stated more formally, is as follows:

H2: The decline in the information content of earnings is longer-lasting following restatements made to correct revenue recognition errors than for other reasons to restate.

Some restatements are likely to cause greater concern about the credibility of subsequently released financial information (e.g., restatements to correct inaccurate accounting for an off-balance sheet entity). I use the change in stock price at the restatement announcement as a proxy for the market's level of concern on the impact of the restatement for future earnings. I hypothesize that the duration of the decline in the information content of earnings will be longer for firms with a larger (i.e., more negative) stock price decline at the restatement announcement date because it takes longer for the market to reassess the credibility of reported earnings and view earnings as having high information content in the post-restatement period. For example, the stock price of PNC Financial Services Group, Inc. declined significantly upon announcement of a restatement in 2002 to correct its accounting for off-balance sheet entities in which the company held ownership interests. Following the restatement announcement, one analyst commented that "the company's credibility with Wall Street has suffered ... it affects their reputation" (Mandaró 2002). Therefore, the third hypothesis, stated in alternative form, is:

H3: The decline in the information content of earnings is longer-lasting following restatements when the stock price reaction at the restatement announcement date is more severe than when the market reaction is smaller.

The final two hypotheses examine whether firm-level actions to restore investor confidence in the quality of the financial reporting process affect the duration of the decline in the information content of earnings. As the restatements in this sample reflect accounting failures in previously reported information, these firms might intentionally provide signals to the market that they have improved their financial reporting systems. I examine two specific actions taken by firms following restatements that would signal increases in reporting quality: dismissal of the firm's auditor or a change in top management.

Johnson and Lys (1990) find that in equilibrium auditors and companies align themselves based on the quality and services of the audit firm and the needs of the client. Thus, a change in auditor is expected when the equilibrium conditions of either party have changed (e.g., in the context of earnings restatements, a decrease in the quality of the auditor or increase in the risk level of the client). Since the pre-restatement auditor would be viewed as being of low quality for having permitted the erroneous accounting that led to the restatement, dismissal of the auditor could signal the firm's commitment to high-quality financial reporting going forward. Accordingly, I propose the fourth hypothesis:

- H4:** The duration of the decline in the information content of earnings is shorter following restatements where the auditor is dismissed relative to restatements where the auditor is not dismissed.

Given the adverse consequences suffered by firms following earnings restatements (e.g., large decline in stock price, increase in cost of capital, etc.), firms may effect a managerial change in order to restore investor confidence in the quality of reported financial information. The restatement literature provides evidence that earnings restatements are often followed by a change in top management (Desai et al. 2006). In addition, anecdotal evidence suggests that firms replace top management specifically to regain credibility with investors following earnings restatements. For example, six months after their earnings restatement announcement in 2002, PNC Financial Services Group replaced their CFO. Commenting on the change in management, one analyst stated, "Moving to restore investor confidence shaken by a federal accounting probe, PNC Financial Services Group made another change at the top, replacing its chief financial officer" (Sabatini 2002). Therefore, the fifth hypothesis, stated in alternative form, is:

- H5:** The duration of the decline in the information content of earnings is shorter following restatements where top management is replaced relative to restatements where top management is not replaced.

The fourth and fifth hypotheses emphasize changes in the governance of financial reporting following restatements, and are related to a growing body of literature that examines firm-level changes in monitoring mechanisms following accounting failures. Farber (2005) shows that firms that have committed financial reporting fraud take actions to improve their governance following fraud detection and that firms experience superior stock price performance following such changes. Other research provides evidence that governance improvements following restatements, including turnover of outside members of the board of directors (Srinivasan 2005) and top management (Desai et al. 2006), are common. However,

III. RESEARCH DESIGN AND SAMPLE SELECTION

I measure the ERC over a multiple-year period surrounding restatements to examine the duration of the decline in the information content of earnings. The following equation is used to test the first hypothesis, with expected signs in parentheses:

where:

The quarters prior to the restatement are $t \in \{-4, -3, -2, -1, 0\}$, where $t = 0$ is the quarter immediately before the restatement, and the six quarters following the restatement are denoted $t \in \{1, 2, 3, 4, 5, 6\}$. The ERC for $t \in \{-4, -3, -2, -1, 0\}$ is the comparative basis for values of the ERC in the post-restatement period, and is represented by the coefficient on UE_{it} (β_1). I expect the coefficient on UE to be positive based on the well-documented positive relation between earnings surprise and change in price. Coefficient estimates that represent changes in the ERC following the restatement (e.g., $\sum_{t=1}^6 \beta_{2,t}$) are of interest in

testing the first hypothesis. If investors believe that earnings released following restatements are noisy representations of the firm's economic performance and are poor indicators of the firm's expected future cash flows, then the ERC for the post-restatement quarterly earnings announcements will be relatively lower than the ERC for the announcements made prior to the restatement. I expect the coefficient on each of the post-restatement interaction terms (e.g., $\sum_{i=1}^6 \beta_{2,i}$) to be significantly negative if there is a long-lasting decline in the information content of earnings following restatements. The ERC is estimated for at least one year before and after the restatement to mitigate the influence of seasonal patterns in the market's response to earnings announcements.⁵

⁵ The choice to terminate analysis of the ERC in the pre-restatement period with quarter $t = -4$ and in the post-restatement period with quarter $t = 6$ is *ad hoc*, though analysis of additional quarters beyond those examined in this paper (specifically, quarters $t = -8, t = -7, t = -6, t = -5$ and $t = 7, t = 8$) provide results consistent with the ERC pattern that is reported in Table 4.

UR_{it} is the cumulative abnormal return surrounding the earnings announcement for firm i at quarter t , measured over a three-day window, where the abnormal return is the firm's return less the CRSP value-weighted market return.⁶ QTR_{it} is an indicator variable equal to 1 if firm i 's earnings announcement belongs to quarter t , and 0 otherwise. UE_{it} is unexpected quarterly earnings for firm i at quarter t 's announcement date, scaled by price as of the end of the quarter for which earnings are announced, where expected earnings is based on the median of analyst forecasts outstanding within 60 days prior to the day before the earnings announcement.⁷ I collect firm-level data and earnings report dates from Compustat, price and stock return data from CRSP, and realized earnings and earnings forecasts from I/B/E/S.⁸ At a minimum, I require that firms have sufficient data to calculate regression variables for the quarters immediately before and after the restatement to facilitate pre- to post-restatement comparison of the ERC. Additional earnings announcements for each restatement observation, from four quarters before time $t = 0$ through six quarters afterward, are included when data are available.

The persistence of earnings and various firm-level characteristics (e.g., growth, risk, and size) systematically affect the relation between unexpected returns and unexpected earnings (Kothari 2001). Therefore, $CONTROLS_{it}$ is a vector of seven control variables included in the regression to mitigate these influences on measurement of the ERC (e.g., as in Subramanyam and Wild [1996], Raedy and Rock [2003], Blouin et al. [2003], among

⁶ Assessment of the market's reaction to earnings by measuring price changes over a three-day period, as in Equation (1), is the appropriate research design for examining the information content of earnings over time as it measures the market's responsiveness to information contained in earnings announcements (Collins and Kothari 1989). However, since the independent variable UE is measured using information from a longer window of time than the three-day measurement period of the dependent variable UR , there is error in measuring expected earnings since most of the earnings forecasts are made before the day prior to the earnings announcement. This measurement error is mitigated if an additional independent variable that is correlated with the measurement error in unexpected earnings but uncorrelated with the dependent variable is included in the regression. Following Easton and Zmijewski (1989), the stock return for each firm measured from the day of the earliest analyst forecast through two days before each earnings announcement is included in sensitivity tests of Equation (1) (results not tabulated). The coefficient on the stock return variable is negative, indicating that the variable is useful in reducing measurement error in UE . Inferences from the results of regressions including this control variable are unaltered from those reported in the paper.

⁷ Results are robust when UE is scaled by price measured at the beginning of the quarter for which earnings are announced or with price measured on the date of the most recent earnings forecast for the quarter. In addition, papers such as Brown (2001) and Bartov et al. (2002) suggest that the forecast closest in time to the earnings announcement date is a superior proxy of the market's expectation of earnings relative to the median of outstanding forecasts within a specific date range. A robustness check using the most recent analyst forecast rather than the median outstanding forecast provides results that are quantitatively identical to those reported in the paper. In addition, given that the median of the analyst forecasts made within a 60-day window is used as a proxy for the market's expectation of earnings, forecasts used in this calculation for quarter 1 could be comprised of forecasts made before and after the restatement announcement, depending on the timing of the restatement and the surrounding earnings announcements. To ensure that only forecasts made following the restatement are used in calculating expected earnings, forecasts made before the restatement was announced (but that fell within the 60-day window) are removed from the set of forecasts used to calculate expected earnings for quarter 1.

⁸ Realized earnings from I/B/E/S are used as actual earnings in calculating UE in order to match the basis of earnings in the forecasted and reported earnings amounts (Abarbanell and Lehavy 2002). A sensitivity test using earnings before extraordinary items from Compustat as actual earnings results in a qualitatively similar pattern of the decline in the information content of earnings following a restatement (results not tabulated).

others). All of the control variables are measured with respect to each quarterly earnings announcement, and thus vary for each firm-quarter observation.⁹

NONLINEAR is defined as $UE * |UE|$ and is a control for nonlinearity in the price-earnings relation (Freeman and Tse 1989; Subramanyam 1996; Lipe et al. 1998). Since extreme values of unexpected earnings are less value-relevant, I expect the coefficient on this variable to be negative. *PREDICT* is defined as the variance of the absolute values of unexpected earnings over the two-year period prior to the earnings announcement, where unexpected earnings are based on a seasonal random walk.¹⁰ Lipe (1990) finds that a negative relation exists between unexpected returns and the predictability of earnings, thus I expect the coefficient on *PREDICT*, when interacted with *UE*, will be negative. *PERSIST* is the autoregressive coefficient from Foster's (1977) model estimated over the two-year period prior to the earnings announcement. Based on evidence of a positive relation between earnings response coefficients and earnings persistence (Kormendi and Lipe 1987; Easton and Zmijewski 1989), I expect that the coefficient on *PERSIST*, when interacted with *UE*, will be positive. *MTB* is the market-to-book-value ratio as of the end of the quarter for which the earnings announcement is made. Given the positive relation between earnings response coefficients and growth (Collins and Kothari 1989), I expect that the coefficient on *MTB* interacted with *UE* will be positive. *BETA* is the market-model beta estimated over the year prior to the earnings announcement ending two days prior to the announcement date. I expect that the coefficient on *BETA* will be negative when interacted with *UE* based on the negative relation between earnings response coefficients and a security's level of systematic risk (Collins and Kothari 1989; Easton and Zmijewski 1989). *SIZE* is the natural log of the market value of equity, in millions of dollars, as of the quarter for which the earnings announcement is made. No prediction is made as to the direction of this control since size is likely to be highly correlated with other firm-level characteristics. *LOSS* is an indicator variable equal to 1 if reported earnings per share is less than 0, and 0 otherwise. Given that negative earnings have lower information content (Hayn 1995), I expect that the coefficient on *LOSS* will be negative when interacted with *UE*. Finally, *Q4* is an indicator variable equal to 1 if the earnings announcement is for the fourth quarter of the firm's fiscal year, and 0 otherwise. I expect that the coefficient on this variable will be negative when interacted with *UE* because of the lower information content of fourth-quarter earnings reports (e.g., Mendenhall and Nichols 1988; Salamon and Stober 1994).

The Information Content of Earnings: Subsample Analyses

I use a similar research design to test the second and third hypotheses, which examine the duration of the decline in the information content of earnings across different subsets of firms. The following regression is used in these tests:

⁹ Though period-specific measurement of the controls seems most appropriate in Equation (1), I performed several robustness checks using alternate definitions of the control variables to ensure results are not unduly influenced by the time reference used. Alternative specifications of controls include (1) control variables measured before the restatement for all quarters, (2) control variables measured after the restatement for all quarters, and (3) control variables measured either before (for quarters $t = -4$ through $t = 0$) or after the restatement (for quarters $t = 1$ through $t = 6$). Patterns of the information content of earnings surrounding restatements from these analyses are quantitatively similar to results reported in the paper across each of these specifications.

¹⁰ A potential alternate definition of *PREDICT* is based on the mean of *UE* over the two years prior to the earnings announcement, where unexpected earnings are based on analyst forecasts rather than time-series properties. Results are unaltered from those reported in the paper when *PREDICT* is calculated using analyst forecasts as the basis for expected earnings.

$$\begin{aligned}
UR_{it} = & \alpha_1 + \sum_{i=1}^6 \alpha_{2,i} QTR_{it} + \sum_{i=1}^6 \alpha_{3,i} [QTR_{it} * SUB_i] + \beta_1 UE_{it} \quad (+) \\
& + \sum_{i=1}^6 \beta_{2,i} [UE_{it} * QTR_{it}] + \beta_3 [UE_{it} * SUB_i] + \sum_{i=1}^6 \beta_{4,i} [UE_{it} * SUB_i * QTR_{it}] \quad (-) \\
& + \beta_5 NONLINEAR_{it} + \sum_{k=6}^{12} \beta_k CONTROLS_{it} \quad (-) \\
& + \sum_{k=13}^{19} \beta_k [UE_{it} * CONTROLS_{it}] + \varepsilon_{it} \quad (2)
\end{aligned}$$

where *UE*, *QTR*, *NONLINEAR*, and *CONTROLS* are as defined previously. In tests of the second hypothesis, *SUB_i* is an indicator variable equal to 1 if the observation represents a restatement to correct revenue recognition errors, and 0 otherwise. The coefficient estimates of interest for the second hypothesis are β_3 and $\sum_{i=1}^6 \beta_{4,i}$. If the decline in the information content of earnings is long-lasting following a restatement for restatements to correct revenue recognition errors, then $\beta_3 + \sum_{i=1}^6 \beta_{4,i} < 0$. In tests of the third hypothesis, *SUB_i* is an indicator variable equal to 1 if the restatement caused a large percentage decline in the firm's stock price at the announcement date. Observations are considered to have a large percentage decline (i.e., high CAR) if the three-day cumulative abnormal return around the restatement announcement date is greater (i.e., more negative) than the median for the sample. If the decline in the information content of earnings is long-lasting for firms that experienced a high CAR, then $\beta_3 + \sum_{i=1}^6 \beta_{4,i} < 0$.

The Information Content of Earnings: Influence of Firm-Level Changes

The final two hypotheses examine whether the duration of the decline in the information content of earnings is systematically different for firms that made changes to governance of the financial reporting process following the restatement, relative to those that did not. I estimate the following regression for the post-restatement period:

$$\begin{aligned}
UR_{it} = & \alpha_1 + \sum_{i=1}^6 \alpha_{2,i} QTR_{it} + \alpha_3 CHANGE_{it} + \beta_1 UE_{it} + \sum_{i=1}^6 \beta_{2,i} [UE_{it} * QTR_{it}] \quad (+) \\
& + \sum_{i=1}^6 \beta_{3,i} [UE_{it} * CHANGE_{it} * QTR_{it}] + \beta_4 NONLINEAR_{it} \quad (+) \\
& + \sum_{k=5}^{11} \beta_k CONTROLS_{it} + \sum_{k=12}^{18} \beta_k [UE_{it} * CONTROLS_{it}] + \varepsilon_{it} \quad (3)
\end{aligned}$$

where *UE*, *QTR*, *NONLINEAR*, and *CONTROLS* are as previously defined. In tests of the fourth hypothesis, *CHANGE_{it}* is an indicator variable equal to 1 if the firm dismissed their auditor before the earnings announcement date for quarter *t*, and 0 otherwise. If the decline in the information content of earnings is relatively shorter for firms that dismiss their auditor

relative to firms that do not, then $\sum_{t=1}^6 \beta_{3,t} > 0$. In tests of the fifth hypothesis, $CHANGE_{it}$ is an indicator variable equal to 1 if the firm experienced turnover in top management before the earnings announcement date for quarter t , and 0 otherwise. The coefficient of interest for the fifth hypothesis is $\sum_{t=1}^6 \beta_{3,t}$. If the duration of the decline in the information content of earnings is shorter for firms that replace top management relative to firms that do not replace top management, then $\sum_{t=1}^6 \beta_{3,t} > 0$.¹¹

To define $CHANGE_{it}$ for the fourth hypothesis, I collect auditor information from Compustat for the year prior to the restatement through two years afterward. Inconsistencies in a firm's auditor from year to year are investigated by matching audit-firm changes reported on Form 8-K.¹² Only audit-firm changes that were initiated by firms (i.e., dismissals) as reported on Form 8-K are considered to be auditor switches so that auditor-initiated resignations are not categorized as firm-level changes. To define $CHANGE_{it}$ for the fifth hypothesis, I characterize a change in top management as turnover in the position of CEO, President, CFO, or Chairman following the restatement announcement date. Management personnel are collected from Form 10-K for each year surrounding the restatement, with changes in management verified with press releases obtained from Lexis-Nexis.¹³

Sample

The GAO report provides a comprehensive list of firms that issued earnings restatements between January 1, 1997 and June 30, 2002.¹⁴ According to the GAO report, restatements were identified through a search of press releases and other media coverage using Lexis-Nexis. The restatements included in the database are those resulting from "aggressive" accounting practices, intentional and unintentional misuse of facts applied to financial statements, oversight or misuse of accounting rules, and fraud (GAO 2002, 76). The database excludes restatements resulting from normal corporate activity (e.g., mergers

¹¹ Note that the time period analyzed with Equation (3) consists of the post-restatement period, as auditor dismissals and replacement of top management as an indication of stronger governance occurs following the restatement announcement.

¹² Coopers and Lybrand, LLP (C&L) merged with PriceWaterhouse, LLP (PW) in 1997 to form PricewaterhouseCoopers, LLP (PWC). As Compustat does not distinguish notation between PW and PWC, a firm that used C&L as its auditor in 1996 and continued to use the same firm (which is coded as PWC in 1997 and beyond) would appear to have an inconsistency in audit firm, when in fact there was no switch. These inconsistencies are not counted as auditor switches in this paper.

¹³ The reason for management turnover is not explicitly considered in my analysis, and thus I capture management changes related to the restatement as well as unrelated management changes that coincidentally occurred in the post-restatement period. Inclusion of unrelated turnover will bias against finding results to support the fifth hypothesis as there is no reason turnover unrelated to the restatement would cause systematic differences in the ERC in the post-restatement period.

¹⁴ All of the restatements in this sample were issued prior to enactment of the Sarbanes-Oxley Act of 2002. Though the Sarbanes-Oxley Act was issued in part to improve investor confidence in the quality of financial reporting following a series of high-profile accounting scandals, the imposition of the Sarbanes-Oxley Act, and any potential implications on the market's assessment of the information content of earnings, does not affect inferences drawn from this study.

and acquisitions) or presentation issues (e.g., changes in business segment definitions).¹⁵ The firm name and restatement announcement date are collected from this database and cross-checked against the restatement announcement press releases using Lexis-Nexis. The reason for restatement, amount of restatement, and number of reporting periods affected are gathered from the press release or the restated financial statements.¹⁶

The composition of the sample is presented in Table 1, with sample attrition in Panel A. Of the 919 observations from the GAO database, 229 observations are deleted because the firm is not covered by CRSP (46 firms), Compustat (44 firms), or I/B/E/S (139 firms). In addition, 33 firms are not included because no evidence of restatement could be found, 47 foreign firms are omitted, and 116 firms are missing data from CRSP (36 firms) or Compustat (80 firms).¹⁷ Finally, 279 firms are not included because of missing I/B/E/S data to calculate the market's expectation of earnings for the quarter prior to or following the restatement.¹⁸ The final sample comprises 215 firms that issued earnings restatements.¹⁹ Panel B of Table 1 shows the frequency of earnings announcements. There are 1,767 quarterly earnings announcements in total, which are distributed evenly over the time-series of announcements. Missing data for additional quarters (other than quarters $t = 0$ and $t = 1$) results in less than the full sample of 215 firms included in other pre- and post-restatement periods.²⁰

IV. EMPIRICAL RESULTS

Descriptive Statistics

Descriptive statistics are provided in Table 2, where Panel A provides comparison of pre- and post-restatement values of the regression variables. Four of the control variables are statistically different in the pre- versus post-restatement periods: *PREDICT*, *PERSIST*, *MTB*, and *LOSS*. Specifically, in the post-restatement period the growth rate of sample firms is lower, earnings are less predictable and less persistent, and there is a greater frequency

¹⁵ As a general rule, the database does not include restatements due to changes in accounting policy (e.g., change from FIFO to LIFO accounting). However, the database does include restatements following the issuance of SAB No. 101 (SEC 1999), since SEC officials considered restatements following the issuance of this accounting guidance necessary to correct a previous misstatement of financial reports (GAO 2002, 76). Restatements prompted by adoption of SAB No. 101 are included in the technical restatement category in this paper. The primary results of the paper are qualitatively unaltered if the SAB No. 101 restatement firms are removed from the sample.

¹⁶ The GAO database classifies each restatement into one of 13 categories based on the reason for the restatement. These classifications are not used in this study since some discrepancies between the GAO categorization and the restatement announcement press releases were discovered. Rather, data are hand-collected and the observations were classified into ten categories according to the reason for restatement.

¹⁷ The 80 observations with missing firm-level data from Compustat are for firms that delisted from their trading exchange following the restatement. Of these firms, 11 observations were delisted because of a merger and 69 observations were delisted for performance reasons.

¹⁸ The requirement that firms have analyst forecast information from I/B/E/S is a severe restriction as it results in the loss of more than one-half of the potential sample. However, I estimate earnings using analyst expectations (rather than a seasonal random-walk model, for example) due to the likelihood that a restatement firm's information environment is changing surrounding the time of the restatement and thus the expectation of earnings four quarters ago is no longer a suitable proxy. Results from a sample of 488 restatements with expected earnings defined using a seasonal random-walk model are qualitatively similar to those presented in the paper.

¹⁹ I replicated the research designs used in Anderson and Yohn (2002) and Wu (2002) using my sample to document comparable results of a short-term decline in the information content of earnings following restatements. Though my sample is different from the samples used in these papers in terms of time period and types of restatements examined, I find results consistent with the respective findings of these papers.

²⁰ All 215 observations are included in the analyses to avoid survivorship bias. Sensitivity tests show that the primary results are unaltered when the surviving firms and those firms with missing post-restatement data are analyzed separately.

TABLE 1
Restatement Sample

Panel A: Sample Attrition

	# Observations
Restatement announcements from GAO report	919
Restatement not included because:	
Firm not covered by CRSP, Compustat, or I/B/E/S	(229)
Potential restatement did not result in actual restatement ^a	(33)
Foreign firm ^b	(47)
Missing necessary data from CRSP or Compustat ^c	(116)
Missing necessary data from I/B/E/S ^d	(279)
Final Sample	215

Panel B: Earnings Announcement Frequency

Quarter	# Observations
-4	148
-3	149
-2	160
-1	168
0	215
1	215
2	158
3	156
4	143
5	134
6	121
Total	1,767

The database of earnings restatements from the GAO report is the basis of the sample. The database consists of restatements that were announced between January 1, 1997 and June 30, 2002, and includes restatements resulting from “aggressive” accounting practices, intentional and unintentional misuse of facts, oversight or misuse of accounting rules, and fraud. The database excludes restatements resulting from normal corporate activity or financial statement presentation issues.

^a These observations are excluded because the firms never restated previously reported results. These 33 restatement announcements mentioned the possibility of a future restatement in the initial announcement (often due to an SEC investigation or other accounting inquiry), but a restatement was not subsequently reported.

^b All foreign firms are omitted from the final sample because of the different filing and disclosure requirements required of foreign firms trading ADRs in the U.S. relative to U.S.-domiciled firms.

^c At a minimum, data necessary for computation of regression variables for Equation (1) are required for the two quarters immediately surrounding the restatement announcement. Specifically, 36 observations are missing return data from CRSP for either the quarterly earnings announcement before or after the restatement and 80 observations are missing firm-level data from Compustat for construction of the regression variables for the quarter following the restatement because of delisting from the firm’s trading exchange. Of the firms that delisted, 11 observations delisted because of a merger, and 69 observations delisted for performance reasons.

^d At a minimum, data necessary for computation of regression variables for Equation (1) are required for the two quarters immediately surrounding the restatement announcement. These observations are missing data from I/B/E/S to calculate expected earnings for either the quarter before or the quarter following the restatement.

of reported losses.²¹ Statistics regarding the timing of auditor dismissals and management

²¹ I address the potential influence of these differences in the regression variables in the pre- and post-restatement periods on my research design in the sensitivity analysis later in this section.

TABLE 2
Descriptive Statistics

Panel A: Regression Variables*

Variable	Pre-Restatement Period				
	Mean	Median	Standard Deviation	Q1	Q3
UR	0.003	0.003	0.109	-0.045	0.052
UE	-0.001	0.000	0.015	0.000	0.000
NONLINEAR	0.000	0.000	0.002	0.000	0.000
PREDICT	0.009	0.000	0.144	0.000	0.000
PERSIST	0.626	0.684	0.665	0.273	0.932
MTB	4.28	2.99	3.53	1.75	5.51
BETA	1.06	0.890	0.752	0.544	1.44
SIZE	7.46	7.31	1.73	6.16	8.90
LOSS	0.160	0.000	0.367	0.000	0.000
Q4	0.233	0.000	0.423	0.000	0.000

Variable	Post-Restatement Period				
	Mean	Median	Standard Deviation	Q1	Q3
UR	-0.002	-0.003	0.132	-0.061	0.059
UE	-0.003	0.000	0.022	-0.001	0.001
NONLINEAR	0.000	0.000	0.003	0.000	0.000
PREDICT	0.021***	0.000***	0.201	0.000	0.001
PERSIST	0.522***	0.551***	0.414	0.246	0.839
MTB	3.30***	2.23***	3.17	1.28	4.07
BETA	1.08	0.93	0.694	0.607	1.42
SIZE	7.40	7.30	1.91	5.89	9.01
LOSS	0.257***	0.000***	0.437	0.000	0.000
Q4	0.233	0.000	0.423	0.000	0.000

Panel B: Date of Auditor Dismissal and Management Turnover Relative to Restatement Announcement

Quarter	Auditor Dismissal		Management Turnover	
	# of Observations	% of Observations	# of Observations	% of Observations
1	6	2.79	19	8.84
2	9	4.19	15	6.98
3	7	3.26	23	10.70
4	6	2.79	15	6.98
5	7	3.26	13	6.05
6	7	3.26	8	3.72
Total	42	19.53	93	43.26

(continued on next page)

TABLE 2 (continued)

Panel C: Industry Composition

Industry	# of Observations	% of Observations	% of Compustat Population
Agriculture	0	0	0.32
Mining and Construction	6	2.79	6.52
Manufacturing	83	38.60	33.54
Transportation and Utilities	18	8.37	9.27
Wholesale and Retail Trade	25	11.63	9.26
Finance, Insurance and Real Estate	25	11.63	21.73
Services	58	26.98	17.95
Public Administration	0	0	1.41
Total	215	100%	100%

*** Significantly different between pre- and post-restatement period at 0.01 level (two-tailed)

^a Regression variables are: *UR* is the cumulative abnormal return measured over a three-day window surrounding the earnings announcement, where the abnormal return is the firm's return less the CRSP value-weighted market return; *UE* is the unexpected earnings for each announcement date, scaled by price as of beginning of the quarter for which earnings are announced, where expected earnings is based on the median analyst forecast of quarterly earnings per share outstanding within 60 days prior to the announcement date; *NONLINEAR* is equal to $UE * |UE|$; *PREDICT* is the variance of the absolute values of unexpected earnings over the two-year period prior to the earnings announcement, where expected earnings is based on seasonal random walk; *PERSIST* is the autoregressive coefficient from Foster's (1977) model estimated over the two-year period prior to the earnings announcement; *MTB* is the market-to-book-value ratio; *BETA* is the market-model beta estimated over the one-year period prior to the earnings announcement; *SIZE* is the log of the market value of equity (in millions); *LOSS* is an indicator variable equal to 1 if earnings are less than 0, and 0 otherwise; *Q4* is an indicator variable equal to 1 if the earnings announcement is for the fourth quarter of the firm's fiscal year, and 0 otherwise.

turnover are in Panel B of Table 2. Forty-two firms (19.53 percent) dismissed their auditor during the six quarters following the restatement.²² The mean (median) number of days between the auditor-switch announcement and the restatement announcement was 277 (236) days (not tabulated). A sizeable number of firms replaced at least one member of top management following the restatement (93 firms). Of these, 72 firms replaced top management within the first year and 21 firms had management turnover in the first two quarters of the second year following the restatement.²³ Industry composition is reported in Table 2, Panel C. The largest concentrations of firms are in manufacturing (38.6 percent) and services (26.98 percent). Overall, a wide variety of industries are represented. Industry percentages for the population of Compustat firms in 2000 are also shown as a comparison.

The distribution of restatements by year and type is reported in Table 3, Panel A. The predominant reasons for restatements are technical (19.53 percent), adjustment of expenses

²² Ten of these observations occurred during the Spring 2002, with companies changing from Arthur Andersen, LLP (AA) to another audit firm. As AA was under indictment for alleged obstruction of justice relating to their involvement with Enron Corporation, these ten companies were likely forced to hire a new auditor for reasons unrelated to providing signals of high reporting quality. Sensitivity tests (not tabulated) demonstrate that the results reported in the paper are quantitatively unaltered when AA-related auditor dismissals are not classified as audit-firm changes.

²³ A total of 136 firms (results not tabulated) experienced management turnover within the two-year period following the restatement. This rate of turnover (63.26 percent) is similar to that found in other restatement research (Desai et al. 2006). Desai et al. (2006) find that 60 percent of their sample of restatement firms experience management turnover within two years following a restatement, which is significantly greater than the corresponding turnover rate for their sample of industry-, size-, and age-matched control firms of 35 percent.

TABLE 3
Restatement Characteristics

Panel A: Distribution of Restatements by Year and Type

Reason for Restatement	1997	1998	1999	2000	2001	2002	Total # Obs.	% of Sample
Revenue Recognition	7	5	10	9	4	6	41	19.07
Adjustment of Expenses	6	8	9	7	7	7	44	20.47
Basis of Assets/Investments	1	1	4	0	3	4	13	6.05
Inventory	0	1	0	1	1	1	4	1.86
Acquisition/Merger	2	2	3	2	3	1	13	6.05
Technical*	3	0	3	6	28	2	42	19.53
Miscellaneous errors—fraud or intent to deceive implied ^b	1	4	2	2	3	1	13	6.05
Miscellaneous errors—fraud or intent to deceive not implied ^c	0	0	2	0	0	2	4	1.86
In response to SEC guidance ^d	0	2	15	4	3	5	29	13.48
Other	0	0	3	2	4	3	12	5.58
Total # observations	20	23	51	33	56	32	215	100
% of sample	9.30%	10.70%	23.70%	15.35%	26.06%	14.89%		

Panel B: Firm Restatement Frequency

	One Restatement	Two Restatements	Total Restatements in Sample
Number of Firms	189	26	215

(continued on next page)

TABLE 3 (continued)

Panel C: Statistics by Reason for Restatement^a

Reason for Restatement	# Obs.	Average Announcement Date CAR	Average Duration (in quarters)	Restatement \$ Amount, (in millions)		Restatement \$ Amount, as a Percentage of Firm Assets	
				Mean	Median	Mean	Median
Revenue Recognition	41	-16.17%	6.44	-60.29	-6.30	-4.82	-2.55
Adjustment of Expenses	44	-9.98%	5.59	-16.11	-1.90	-1.90	-0.37
Basis of Assets/Investments	13	-7.32%	4.08	-46.51	-20.00	-0.91	-0.44
Inventory	4	-4.30%	4.50	-5.64	-4.64	-0.80	-0.60
Acquisition/Merger	13	-8.05%	5.77	-30.94	-0.35	0.36	-0.08
Technical ^a	42	-2.42%	5.26	-16.89	0.00	-0.55	0.00
Miscellaneous errors—fraud or intent to deceive implied ^b	13	-23.04%	7.69	-45.39	-13.60	-1.86	-0.81
Miscellaneous errors—fraud or intent to deceive not implied ^c	4	-1.36%	3.50	-15.27	-12.50	-0.22	-0.22
In response to SEC guidance ^d	29	-5.17%	5.03	-16.41	4.40	5.37	1.37
Other	12	-1.24%	3.25	-30.33	-8.20	-0.35	-0.13
Total Sample	215	-8.79%	5.47	-29.82	-2.46	-0.87	-0.34

Restatement data is from the press release issued on the restatement announcement date, where available, or from the restated financial statements.

^a Technical restatements include restatements following the adoption of SAB No. 101 (17 restatements) and the issuance of various EITF Consensus.

^b Restatements are included in this group if the restatement announcement did not state a specific reason for restatement other than the discovery of accounting failures. Restatement announcements for this group were very specific that the cause of the restatement was isolated to specific transactions or the intentional actions of specific employees.

^c Restatements are included in this group if the restatement announcement did not state a specific reason for restatement other than the discovery of accounting errors. Restatement announcements for this group contained specific language to demonstrate that computer error or accidental misreporting of an accounting amount caused the restatement.

^d Restatements in this group include correction of the accounting previously applied under various Statement of Financial Accounting Standards (SFAS), including SFAS No. 125 and SFAS No. 133, among others.

^e The announcement date cumulative abnormal return (CAR) is measured over the three-day period surrounding the restatement announcement. Average duration of the restatement is measured as the number of quarters restated. The restatement percentage is the dollar amount of the restatement divided by the firm's assets as of the quarter prior to the restatement announcement.

(20.47 percent), and correction of revenue recognition errors (19.07 percent). The number of restatements and distribution of restatements over time is similar to that documented in other restatement research (Wu 2002).²⁴ As noted in Panel B of Table 3, most of the observations for the final sample represent distinct firms. However, 13 firms that issued two separate restatements during the sample period are included twice.

Panel C of Table 3 compares restatement statistics across the ten types of restatements. The average cumulative abnormal return for the three-day period surrounding the restatement announcement date is -8.79 percent.²⁵ Note that the categories with the most negative market response (the majority of firms classified as high CAR for the third hypothesis) are miscellaneous accounting failures with fraud implied, revenue recognition, and adjustment of expenses. The amount of restatement is calculated as the difference between originally reported net income less restated net income, summed across all restated periods. The distribution of the magnitude of restatement is highly skewed to the left by large values for the overall sample, as well as within each category. An alternate comparison of cross-sectional differences is the restatement amount expressed as a percentage of firm assets, as shown in the far right columns. The mean (median) value for the sample is -0.87 percent (-0.34 percent), with the highest (i.e., most negative) percentages from the revenue recognition, adjustment of expenses, and miscellaneous errors with fraud-implied categories.

Information Content of Earnings

The first hypothesis posits that the decrease in the information content of earnings following restatements is long-lasting. Table 4 reports the ERC over a multiple-year time period surrounding restatements, where the individual coefficient estimates are reported in the left half of the table and summation of relevant variables for calculation of the ERC for each quarter is reported in the right half of the table.²⁶ The information content of earnings for the pre-restatement period is represented by the 6.66 coefficient on β_1 . The pre-restatement ERC is the base period, which is the direct comparison for the pre- versus post-restatement analyses.²⁷

Results are not consistent with the conjecture that the decline in the information content of earnings is long-lasting. As noted in the right half of Table 4, the ERC for the first three earnings announcements following restatements is significantly lower than the ERC for the base period. In particular, the ERC for the first quarter following the restatement is equal to 5.62 (p-value for difference from the base period of 0.014). The ERC for the second and third quarters following the restatement are equal to 5.43 and are also significantly lower than the base period. However, the ERC is not significantly different from the base period thereafter. Results from Table 4 are presented graphically in Figure 1, which depicts

²⁴ The spike in restatement activity during 1999 and 2001 is related to restatements in response to new GAAP or specific guidance issued by the SEC. The majority of SEC guidance restatements in 1999 were in response to a new interpretation of the reporting of acquired in-process research and development, and the large number of technical restatements in 2001 was in response to SAB No. 101.

²⁵ The mean CAR (-8.79 percent) for the sample is slightly lower than found in other restatement research (range of -9.0 percent to -11.2 percent). When the technical restatement firms are removed from the sample, the mean CAR is -10.3 percent, which is more similar to the mean result found in other studies.

²⁶ The value of unexpected earnings (UE) is winsorized at the 1 percent level to control for outliers in all analyses. Significance levels are reported using White (1980) heteroscedasticity-adjusted standard errors.

²⁷ To verify that the ERC is relatively stable across the pre-restatement quarters, I performed a robustness check using a modified version of Equation (1) to calculate quarter-specific estimates of the ERC for quarters $t \in \{-4, -3, -2, -1, 0\}$. The ERC for these quarters was 7.28, 6.53, 6.57, 7.47, and 6.80, respectively. None of the quarterly ERCs are significantly different across the pre-restatement period, which provides reassurance that the use of quarters $t \in \{-4, -3, -2, -1, 0\}$ is appropriate as the comparative base period for the post-restatement quarters.

TABLE 4
Change in the Information Content of Earnings Surrounding a Restatement

$$UR_{it} = \alpha_1 + \sum_{i=1}^6 \alpha_{2,i} QTR_{it} + \beta_1 UE_{it} + \sum_{i=1}^6 \beta_{2,i} [UE_{it} * QTR_{it}] + \beta_3 NONLINEAR_{it}$$
$$+ \sum_{k=4}^{10} \beta_k CONTROLS_{it} + \sum_{k=11}^{17} \beta_k [UE_{it} * CONTROLS_{it}] + \varepsilon_{it}$$

Individual Estimates			Sum of Estimates		
	Predicted Sign	Coefficient Value	Qtr.	ERC Coefficient	ERC
β_1	+	6.66***	Base Period	β_1	6.66***
$\beta_{2,t=1}$	—	-1.04**	1	$\beta_1 + \beta_{2,t=1}$	5.62***,***
$\beta_{2,t=2}$	—	-1.23**	2	$\beta_1 + \beta_{2,t=2}$	5.43***,***
$\beta_{2,t=3}$	—	-1.23**	3	$\beta_1 + \beta_{2,t=3}$	5.43***,**
$\beta_{2,t=4}$	—	-0.74	4	$\beta_1 + \beta_{2,t=4}$	5.92***
$\beta_{2,t=5}$	—	-0.29	5	$\beta_1 + \beta_{2,t=5}$	6.37***
$\beta_{2,t=6}$	—	-0.28	6	$\beta_1 + \beta_{2,t=6}$	6.38***
Adj. R ²					0.095
Number of Observations					1,767

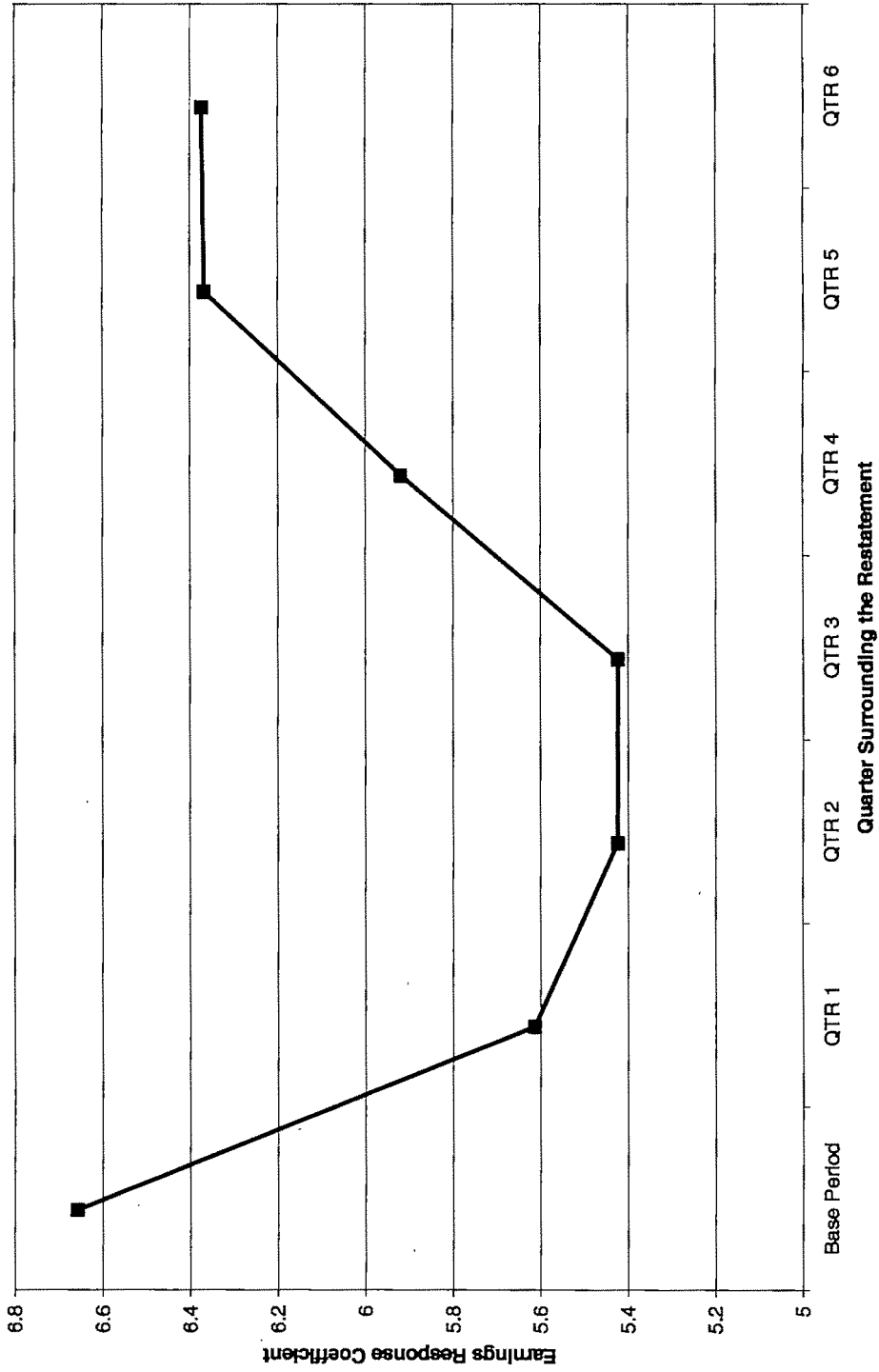
*, **, *** Significantly different from zero at 0.10, 0.05 and 0.01 levels, respectively (two-tailed).
*, **, *** Significantly lower than base period at 0.10, 0.05 and 0.01 levels, respectively (one-tailed).
OLS coefficient estimates are reported for *UE* and significance levels are reported using White (1980) heteroscedasticity-adjusted standard errors.
QTR is an indicator variable equal to 1 if the earnings announcement is for quarter *t*, and 0 otherwise.
All other variables are as defined in Table 2.

the U-shaped pattern of the information content of earnings over time. In general, results for the control variables (not tabulated) have the predicted sign.²⁸

Evidence presented in Table 4 and Figure 1 reflects a temporary decrease in the information content of earnings. Though this is consistent with apprehension about the quality of earnings reported by firms following restatements, the market's concern does not appear to be long-lasting, counter to speculation by the SEC and market analysts.²⁹ Rather, the decrease in the information content of earnings is short-lived, as the ERC rebounds and is not significantly different when measured four or more quarters following a restatement.³⁰

²⁸ The coefficient estimates for the control variables *NONLINEAR* and *PREDICT*, *LOSS*, and *Q4*, when interacted with *UE*, are in the predicted direction and are significant at conventional levels. Coefficients for all other controls are in the predicted direction but are not significant.
²⁹ Choi and Jeter (1992) find that the ERC for quarterly earnings releases immediately following issuance of a qualified audit opinion is significantly lower than the ERC for earnings announcements reported prior to issuance of the qualified opinion. This finding suggests a potential alternative explanation for the results presented in Table 4, in that the post-restatement ERC could be lower because qualified auditor opinions were issued for the sample firms rather than the restatement event inducing the decline in the ERC. I collected the audit opinion for all restatement observations in the sample for the two-year period surrounding the restatement and none of the firms received a qualified audit opinion during this time. Therefore, results presented in this paper cannot be attributed to the issuance of qualified opinions.
³⁰ Though none of the ERC values beyond quarter 3 are significantly lower than the base period, the rebound in the ERC is somewhat incomplete since the ERC for quarters 4, 5, and 6 are slightly lower than the ERC values in the pre-restatement period.

FIGURE 1
Information Content of Earnings Surrounding Restatements



Subsample Analyses

The second and third hypotheses propose that the decline in the information content of earnings will be longer-lasting following restatements to correct revenue recognition errors and for restatements that resulted in a severely negative abnormal return at the announcement date, respectively. Results are presented in Tables 5 and 6. In Table 5 (Table 6), the

TABLE 5
Change in the Information Content of Earnings Following a Restatement:
Revenue Recognition versus Non-Revenue Recognition Restatements

$$UR_{it} = \alpha_1 + \sum_{i=1}^6 \alpha_{2,i} QTR_{it} + \sum_{i=1}^6 \alpha_{3,i} [QTR_{it} * SUB_i] + \beta_1 UE_{it} \sum_{i=1}^6 \beta_{2,i} [UE_{it} * QTR_{it}]$$

$$+ \beta_3 [UE_{it} * SUB_i] + \sum_{i=1}^6 \beta_{4,i} [UE_{it} * SUB_i * QTR_{it}] + \beta_5 NONLINEAR_{it}$$

$$+ \sum_{k=6}^{12} \beta_k CONTROLS_{it} + \sum_{k=13}^{19} \beta_k [UE_{it} * CONTROLS_{it}] + \epsilon_{it} \tag{2}$$

Individual Estimates		Sum of Estimates: Revenue Recognition Restatements			Sum of Estimates: Non-Revenue Recognition Restatements		
	Coefficient Value	Qtr.	ERC Coefficient	ERC	Qtr.	ERC Coefficient	ERC
β_1	6.70***						
$\beta_{2,t=1}$	-0.44	Base Period	$\beta_1 + \beta_3$	7.32***	Base Period	β_1	6.70***
$\beta_{2,t=2}$	-0.73	1	$\beta_1 + \beta_{2,t=1} + \beta_3 + \beta_{4,t=1}$	4.42***,††,###	1	$\beta_1 + \beta_{2,t=1}$	6.26***
$\beta_{2,t=3}$	-0.79	2	$\beta_1 + \beta_{2,t=2} + \beta_3 + \beta_{4,t=2}$	4.86***,###	2	$\beta_1 + \beta_{2,t=2}$	5.97***
$\beta_{2,t=4}$	-0.14	3	$\beta_1 + \beta_{2,t=3} + \beta_3 + \beta_{4,t=3}$	4.52***,###	3	$\beta_1 + \beta_{2,t=3}$	5.91***
$\beta_{2,t=5}$	-0.43	4	$\beta_1 + \beta_{2,t=4} + \beta_3 + \beta_{4,t=4}$	7.65***	4	$\beta_1 + \beta_{2,t=4}$	6.56***
$\beta_{2,t=6}$	-0.12	5	$\beta_1 + \beta_{2,t=5} + \beta_3 + \beta_{4,t=5}$	7.47***	5	$\beta_1 + \beta_{2,t=5}$	6.27***
β_3	0.62	6	$\beta_1 + \beta_{2,t=6} + \beta_3 + \beta_{4,t=6}$	6.83***	6	$\beta_1 + \beta_{2,t=6}$	6.58***
$\beta_{4,t=1}$	-2.46***						
$\beta_{4,t=2}$	-1.73***						
$\beta_{4,t=3}$	-2.01**						
$\beta_{4,t=4}$	0.47						
$\beta_{4,t=5}$	0.60						
$\beta_{4,t=6}$	-0.37						
Adj. R ²							0.094
Number of observations							1,767

*** Significant different from zero at 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
Significant lower than base period at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
††† Significant different between groups at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
OLS coefficient estimates are reported for UE and significance levels are reported using White (1980) heteroscedasticity-adjusted standard errors.
SUB is an indicator variable equal to 1 if the observation is a revenue recognition restatement, and 0 otherwise. Revenue recognition restatements are identified from the firm's restatement announcement press release or the restated financial statements.
All other regression variables are as defined in Table 2.

TABLE 6
Change in the Information Content of Earnings Following a Restatement:
High CAR versus Low CAR Market Reaction

$$UR_{it} = \alpha_1 + \sum_{t=1}^6 \alpha_{2,t} QTR_{it} + \sum_{t=1}^6 \alpha_{3,t} [QTR_{it} * SUB_i] + \beta_1 UE_{it} + \sum_{t=1}^6 \beta_{2,t} [UE_{it} * QTR_{it}]$$

$$+ \beta_3 [UE_{it} * SUB_i] + \sum_{t=1}^6 \beta_{4,t} [UE_{it} * SUB_i * QTR_{it}] + \beta_5 NONLINEAR_{it}$$

$$+ \sum_{k=6}^{12} \beta_k CONTROLS_{it} + \sum_{k=13}^{19} \beta_k [UE_{it} * CONTROLS_{it}] + \epsilon_{it} \tag{2}$$

Individual Estimates		Sum of Estimates: High CAR Market Reaction			Sum of Estimates: Low CAR Market Reaction		
Coefficient Value		Qtr.	ERC Coefficient	ERC	Qtr.	ERC Coefficient	ERC
β_1	6.60***						
$\beta_{2,t=1}$	-0.44	Base Period	$\beta_1 + \beta_3$	7.20***	Base Period	β_1	6.60***
$\beta_{2,t=2}$	-0.98	1	$\beta_1 + \beta_{2,t=1} + \beta_3 + \beta_{4,t=1}$	5.53***,###	1	$\beta_1 + \beta_{2,t=1}$	6.16***
$\beta_{2,t=3}$	-1.07	2	$\beta_1 + \beta_{2,t=2} + \beta_3 + \beta_{4,t=2}$	5.39***,###	2	$\beta_1 + \beta_{2,t=2}$	5.62***
$\beta_{2,t=4}$	-0.89	3	$\beta_1 + \beta_{2,t=3} + \beta_3 + \beta_{4,t=3}$	5.29***,###	3	$\beta_1 + \beta_{2,t=3}$	5.53***
$\beta_{2,t=5}$	0.25	4	$\beta_1 + \beta_{2,t=4} + \beta_3 + \beta_{4,t=4}$	5.94***	4	$\beta_1 + \beta_{2,t=4}$	5.71***
$\beta_{2,t=6}$	-0.22	5	$\beta_1 + \beta_{2,t=5} + \beta_3 + \beta_{4,t=5}$	6.33***	5	$\beta_1 + \beta_{2,t=5}$	6.85***
β_3	0.60	6	$\beta_1 + \beta_{2,t=6} + \beta_3 + \beta_{4,t=6}$	7.14***	6	$\beta_1 + \beta_{2,t=6}$	6.38***
$\beta_{4,t=1}$	-1.23*						
$\beta_{4,t=2}$	-0.83						
$\beta_{4,t=3}$	-0.84						
$\beta_{4,t=4}$	0.63						
$\beta_{4,t=5}$	-1.12						
$\beta_{4,t=6}$	0.16						
Adj. R ²							0.090
Number of observations							1,767

*, **, *** Significantly different from zero at 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
*, **, *** Significantly different from base period at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
+, ++, +++ Significantly different between groups at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
OLS coefficient estimates are reported for UE and significance levels are reported using White (1980) heteroscedasticity-adjusted standard errors.
SUB is an indicator variable equal to 1 if the observation is a high CAR market reaction restatement, and 0 otherwise. High CAR market reaction restatements are those for which the three-day cumulative abnormal return surrounding the restatement announcement date is above the median CAR (i.e., more negative) for the sample. All other regression variables are as defined in Table 2.

individual coefficient estimates are presented in the far left column, with the ERC for the revenue recognition (high CAR) restatements in the middle column and the ERC for the non-revenue recognition (low CAR) restatements at the far right column. I draw inferences regarding the duration of the decline in the information content of earnings primarily through comparison of the ERC within each subgroup over time, although I note differences between subgroups as well.

As shown in the middle column of Table 5, the ERC for the quarterly earnings announcements immediately following revenue recognition restatements is significantly lower than the base period for the first three post-restatement quarters. The ERC for the base period is equal to 7.32, and then declines significantly for three periods before returning to pre-restatement levels by the fourth quarter following the restatement. The short-term decline in the information content of earnings for the revenue recognition-related restatements is consistent with investor skepticism regarding subsequently reported earnings for these firms. As noted in the far right column however, the ERC is not significantly different in the post-restatement period for the non-revenue recognition restatements, indicating credibility of post-restatement earnings is not compromised for these firms. Comparing across groups, the information content of earnings is significantly lower for revenue recognition restatements for only one quarter following restatements. Specifically, the ERC for quarter 1 for the revenue restatement observations is equal to 4.42 versus 6.26 for all other categories of restatements. The F-statistic for the difference between the values is 5.36 and has a one-tailed p-value of 0.0103 (not tabulated). Beyond the first quarter, however, none of the post-restatement ERCs are significantly different between the subsamples.

Table 6 presents results of tests of the third hypothesis. Firms in the high CAR group include those for which the three-day CAR at the restatement announcement date is greater (i.e., more negative) than the median (-4.1 percent) for the sample. As shown in the middle column, the duration of the decline in the information content of earnings is three quarters for the high CAR group. However, the post-restatement ERC is not significantly different from the base period for the low CAR group, indicating the information content of earnings is not affected by the restatement for firms that experienced a milder stock price reaction at the restatement announcement. Overall, these results show that the information content of earnings is only compromised for firms with a severely negative market reaction at the restatement announcement. Comparing across groups, the ERC is not significantly different between the two groups in any of the time periods surrounding the restatement.

Influence of Firm-Level Changes

The fourth and fifth hypotheses posit that the duration of the decline in the information content of earnings is shorter for firms that initiate changes suggestive of a commitment to high-quality reporting. Results are reported for the influence of auditor dismissal in Table 7 and for the influence of management turnover in Table 8. As in the subsample analyses, I focus on comparing the ERC within each group over time, though I also note differences between the groups.

Results in Table 7 show a striking difference between the duration of the decline in the information content of earnings for firms that did and did not dismiss their auditor. As shown in the far right column, the ERC for the group of firms that did not dismiss their auditor is significantly lower than the base period for three quarters following restatements. This pattern is consistent with the short-term decrease in the information content of earnings observed for the entire sample and suggests that the market views earnings reported in the short-term from these firms as having lower information content relative to the pre-restatement period. However, as shown in the middle column, the ERC is not significantly different in any of the post-restatement quarters for the group of firms that dismissed their auditor. The lack of decline in the response to earnings for firms that dismissed their auditor

TABLE 8
The Influence of Management Turnover on the Information Content of Earnings
Following a Restatement

$$UR_{it} = \alpha_1 + \sum_{t=1}^6 \alpha_{2,t}QTR_{it} + \alpha_3CHANGE_{it} + \beta_1UE_{it} + \sum_{t=1}^6 \beta_{2,t}[UE_{it} * QTR_{it}]$$

$$+ \sum_{t=1}^6 \beta_{3,t}[UE_{it} * CHANGE_{it} * QTR_{it}] + \beta_4NONLINEAR_{it}$$

$$+ \sum_{k=5}^{11} \beta_kCONTROLS_{it} + \sum_{k=12}^{18} \beta_k[UE_{it} * CONTROLS_{it}] + \epsilon_{it} \tag{3}$$

Individual Estimates		Sum of Estimates: Firms with Change in Management			Sum of Estimates: Firms with No Change in Management		
	Coefficient Value	Qtr.	ERC Coefficient	ERC	Qtr.	ERC Coefficient	ERC
β_1	5.38***						
$\beta_{2,t=1}$	-1.45***	Base Period	β_1	5.38***	Base Period	β_1	5.38***
$\beta_{2,t=2}$	-1.12**	1	$\beta_1 + \beta_{2,t=1} + \beta_{3,t=1}$	6.24***,†	1	$\beta_1 + \beta_{2,t=1}$	3.93***,###
$\beta_{2,t=3}$	-2.24**	2	$\beta_1 + \beta_{2,t=2} + \beta_{3,t=2}$	5.91***,†	2	$\beta_1 + \beta_{2,t=2}$	4.26***,##
$\beta_{2,t=4}$	-0.70	3	$\beta_1 + \beta_{2,t=3} + \beta_{3,t=3}$	4.95***	3	$\beta_1 + \beta_{2,t=3}$	3.14***,##
$\beta_{2,t=5}$	-1.54	4	$\beta_1 + \beta_{2,t=4} + \beta_{3,t=4}$	5.51***	4	$\beta_1 + \beta_{2,t=4}$	4.68***
$\beta_{2,t=6}$	-0.35	5	$\beta_1 + \beta_{2,t=5} + \beta_{3,t=5}$	4.79***	5	$\beta_1 + \beta_{2,t=5}$	3.84***
$\beta_{3,t=1}$	2.31	6	$\beta_1 + \beta_{2,t=6} + \beta_{3,t=6}$	5.22***	6	$\beta_1 + \beta_{2,t=6}$	5.03***
$\beta_{3,t=2}$	1.65						
$\beta_{3,t=3}$	1.81						
$\beta_{3,t=4}$	0.83						
$\beta_{3,t=5}$	0.96						
$\beta_{3,t=6}$	0.19						
Adj. R ²							0.091
Number of observations							1,142

*, **, *** Significantly different from zero at 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
###,*** Significantly different from base period at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
†,††,††† Significantly different between groups at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
OLS coefficient estimates are reported for *UE* and significance levels are reported using White (1980) heteroscedasticity-adjusted standard errors.
CHANGE is an indicator variable equal to 1 if the firm experienced turnover in management before the earnings announcement date for quarter *t*. Management turnover is defined as a change in the CEO, President, Chairman, or CFO.
All other regression variables are as defined in Table 2.

Results presented in Table 8 incorporating influence of management turnover are generally consistent with results in Table 7. As shown in the middle column, results from the time-series analysis for firms that had management turnover indicate that the ERC is not significantly different from the base period for any of the earnings announcements following restatements. On the other hand, the ERC is significantly lower for the first three quarters following restatements for firms that did not make changes to the firm's top management team. Comparison between the groups shows that the ERC is relatively greater for firms

that replaced management within the first two quarters following restatements, but that the difference between groups is not significant thereafter.

Overall, the results presented in Tables 7 and 8 are consistent with the notion that firms that initiated changes in financial reporting governance report earnings with relatively greater information content following restatements compared with firms that did not alter their governance structures. In fact, the information content of earnings is not compromised following restatements for firms that dismissed their auditor or changed management. Furthermore, for firms that did not effect a governance change, the decline in the information content of earnings is not sustained beyond three quarters following restatements, indicating the information content of earnings is not compromised in the long-term.

Sensitivity Analysis

Results presented thus far indicate that the decline in the information content of earnings following restatements is not long-lasting, contrary to speculation by SEC officials and market analysts. However, given the largely consistent and short-term pattern of changes in the ERC, an alternative explanation for this result could be that time trends in the market's response to earnings news are driving the short-term decline in the ERC. Given that earnings restatements first became a prominent phenomenon during this sample period when a large number of accounting frauds and restatements made headlines worldwide, it is possible that the short-term decline is due to market skepticism of earnings news in general, and not in response to the specific restatements announced by this group of firms.

To mitigate concern that time-period trends affect my results, I conduct tests of the first hypothesis for a matched-sample of non-restatement firms. Specifically, I gather a sample of control firms that did not announce a restatement between January 1, 1997 and June 30, 2002, where control firms are matched individually to each restatement firm based on two-digit SIC code and reporting period (year and quarter of each earnings announcement). I then select the control firm that most closely matches the restatement firm based on predictability of earnings (*PREDICT*). I utilize the predictability of earnings as a match criteria since *PREDICT* decreased significantly from the pre- to post-restatement period for the restatement firms (Table 2, Panel A).³² I utilize the following regression for this analysis:

$$\begin{aligned}
 UR_{it} = & \alpha_1 + \sum_{t=1}^6 \alpha_{2,t} QTR_{it} + \alpha_3 RESTAT_{it} + \beta_1 UE_{it} + \beta_2 [UE_{it} * RESTAT_{it}] \\
 & \hspace{15em} (+) \hspace{15em} (?) \\
 & + \sum_{t=1}^6 \beta_{3,t} [UE_{it} * QTR_{it}] + \sum_{t=1}^6 \beta_{4,t} [UE_{it} * QTR_{it} RESTAT_{it}] \\
 & \hspace{10em} (-) \hspace{10em} (-) \\
 & + \beta_5 NONLINEAR_{it} + \sum_{k=6}^{12} \beta_k CONTROLS_{it} \\
 & + \sum_{k=13}^{19} \beta_k [UE_{it} * CONTROLS_{it}] + \varepsilon_{it} \hspace{10em} (4)
 \end{aligned}$$

³² In separate sensitivity analyses, I collected two additional independently matched samples based on three-digit SIC code, reporting period, and either *PERSIST* or *MTB*. I chose *PERSIST* and *MTB* as the basis of additional control samples because the persistence of earnings and growth rate of the restatement sample firms changed in the pre-versus post-restatement period (Table 2, Panel A). Results from these additional sensitivity analyses are quantitatively robust to results reported in Table 9. These results provide reassurance that differences in the controls in the pre-versus post-restatement periods are not unduly affecting the results.

where *QTR*, *UE*, *NONLINEAR*, and *CONTROLS* are as previously defined. *RESTAT_{it}* is an indicator variable equal to 1 if the observation is from the restatement sample, and 0 if the observation is from the control sample. The control sample is comprised of 215 individual firms, with 2,098 quarterly earnings announcements across quarters *t* = -4 through *t* = 6 and the restatement sample is comprised of 1,767 earnings announcement quarters, for a total of 3,865 quarterly earnings announcements. Results are reported in Table 9.

TABLE 9
Change in the Information Content of Earnings:
Control Sample of Non-Restatement Firms

$$UR_{it} = \alpha_1 + \sum_{t=-1}^6 \alpha_{2,t}QTR_{it} + \alpha_3RESTAT_{it} + \beta_1UE_{it} + \beta_2[UE_{it} * RESTAT_{it}]$$
$$+ \sum_{t=-1}^6 \beta_{3,t}[UE_{it} * QTR_{it}] + \sum_{t=-1}^6 \beta_{4,t}[UE_{it} * QTR_{it} * RESTAT_{it}] + \beta_5NONLINEAR_{it}$$
$$+ \sum_{k=6}^{12} \beta_kCONTROLS_{it} + \sum_{k=13}^{19} \beta_k[UE_{it} * CONTROLS_{it}] + \varepsilon_{it}$$

Individual Estimates		Sum of Estimates: Restatement Sample			Sum of Estimates: Control Sample		
	Coefficient Value	Qtr.	ERC Coefficient	ERC	Qtr.	ERC Coefficient	ERC
β ₁	4.86***						
β ₂	1.30***	Base Period	β ₁ + β ₂	6.16***	Base Period	β ₁	4.86***
β _{3,t=1}	1.56	1	β ₁ + β ₂ + β _{3,t=1} + β _{4,t=1}	5.04***,###	1	β ₁ + β _{3,t=1}	6.42***
β _{3,t=2}	0.82	2	β ₁ + β ₂ + β _{3,t=2} + β _{4,t=2}	5.40***,##	2	β ₁ + β _{3,t=2}	5.68***
β _{3,t=3}	1.01	3	β ₁ + β ₂ + β _{3,t=3} + β _{4,t=3}	4.72***,##	3	β ₁ + β _{3,t=3}	5.87***
β _{3,t=4}	0.97	4	β ₁ + β ₂ + β _{3,t=4} + β _{4,t=4}	5.37***,#	4	β ₁ + β _{3,t=4}	5.83***
β _{3,t=5}	0.22	5	β ₁ + β ₂ + β _{3,t=5} + β _{4,t=5}	6.01***	5	β ₁ + β _{3,t=5}	5.08***
β _{3,t=6}	0.94	6	β ₁ + β ₂ + β _{3,t=6} + β _{4,t=6}	5.70***	6	β ₁ + β _{3,t=6}	5.80***
β _{4,t=1}	-2.68***						
β _{4,t=2}	-1.58**						
β _{4,t=3}	-2.45**						
β _{4,t=4}	-1.76**						
β _{4,t=5}	-0.37						
β _{4,t=6}	-1.40						
Adj. R ²							0.070
Number of observations							3,865

*, **, *** Significantly different from zero at 0.10, 0.05, and 0.01 levels, respectively (two-tailed).
#, ##, ### Significantly different from base period at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
†, ††, ††† Significantly different between groups at 0.10, 0.05, and 0.01 levels, respectively (one-tailed).
OLS coefficient estimates are reported for *UE* and significance levels are reported using White (1980) heteroscedasticity-adjusted standard errors.
This analysis utilizes a matched-sample of firms that did not restate earnings between January 1, 1997 and June 30, 2002. Firms in the control sample are matched based on two-digit SIC code, reporting period, and predictability of earnings (*PREDICT*).
All regression variables are as defined in Table 2, with reference to the restatement date of the sample firm.

As shown in the far right column, the information content of earnings is not significantly different across time for the control firms. Though there is some volatility in the ERC for this sample (e.g., the ERC values range from a high of 6.42 in quarter 1 to a low of 4.86 in the base period), the ERC for each quarter is not statistically different from any of the other quarters (one-tailed p -value for difference in the ERC of the base period versus quarter 1, the largest difference in magnitude, = 0.3358). Furthermore, no pattern of successive increase or decrease in the ERC is noted over time for the control sample. On the other hand, results for the restatement sample are largely consistent with those reported in Table 4, though the duration of the decline in the information content of earnings is slightly longer using this research design. In particular, the ERC is significantly different from the base quarter through the fourth quarter following the restatement (one-tailed p -value = 0.0922 for difference of the fourth quarter from the base period). Overall, the contrasting patterns between the restatement firms and the matched control samples provides assurance that the short-term decline in the information content of earnings is a result of earnings restatements and is not caused by time-period trends.

V. CONCLUSION

The increase in earnings restatements over the last decade has caused concern among market regulators and analysts that investor skepticism regarding financial information is widespread and ongoing following restatements. I examine whether the decline in the information content of earnings is long-lasting by measuring the earnings response coefficient (ERC) over a multiple-year period surrounding restatements. Results indicate that investors perceive post-restatement earnings to have relatively less information content. However, the earnings response coefficients for quarterly earnings announcements exhibit a U-shaped pattern in which they return to near pre-restatement levels over an average of four quarters, suggesting that the market's concern regarding subsequently reported earnings is transitory.

Furthermore, the decrease in information content is more pervasive for firms that corrected errors of revenue recognition and for firms that experienced a severely negative stock price decline at the restatement announcement. Firms that restated earnings and then quickly dismissed their auditor or changed top management did not suffer a decline in the short-term information content of earnings, as did firms that did not make governance changes immediately following the restatement. This result is consistent with investors placing less emphasis on subsequently released earnings for firms that retained the auditor and top management team that was in place at the time of the restatement.

This paper contributes to our understanding of consequences suffered by firms that restate historical results by examining the duration of the decline in the information content of earnings, which is a potentially dramatic outcome of restatements not previously addressed by the literature. Evidence suggests that the information content of earnings is indeed compromised, albeit rather temporarily, by earnings restatements. Inferences drawn from this study are important for policy makers and market regulators in that results do not indicate that firms that issue restatements suffer severe and ongoing credibility problems. Future research in this area can examine whether the Sarbanes-Oxley Act of 2002, which was enacted in part to increase investor confidence in financial reporting, mitigates the decline of the information content of earnings following restatements.

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Management, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

TIM BUSH, *"Divided by Common Language," Where Economics Meets the Law: US versus non-US Financial Reporting Models* (London, U.K.: The Institute of Chartered Accountants in England and Wales, 2005, ISBN 978-1-84152-359-0, pp. 58).

In this "viewpoint" monograph, Tim Bush, a Chartered Accountant with Hermes Pensions Management, puts forth an intriguing thesis, that U.S. and U.K. disclosure laws and practices are more different than they are alike due to important differences in their respective legal systems. Because the Securities Act of 1933 was modeled on the U.K. Companies Act of the day, the two systems might seem to be, and have been described as, very similar. But, he writes, they are really not. U.K. law is based on the duty of a (limited liability) company, regardless of whether its securities are publicly traded, to its shareholders. Consequently, U.K. accounting and auditing is directed toward stewardship—providing shareholders with information about the state of resources entrusted to the managers and directors and how efficiently their company has been run. Emphasis in the U.K. Companies Acts, therefore, has been on the balance sheet.

In contrast, Bush states that corporate law in the United States is governed by the individual states. Because of "a constitutional quirk, the US federal reporting model does not address in enforceable law the fundamental capitalist proposition 'do the accounts show how efficiently a company is run on its capital resources?'" (p. 2). Instead, U.S. federal securities laws must be and are concerned with securities markets and trades by investors in those markets. The consequences of this difference are profound, he writes: "Instead of a commonsense commercial view of what is relevant accounting and truthful reporting for shareholders, the US regime has become rigid and rules based" (p. 3).

Before considering Bush's elaboration of the effects on accounting rules and auditing practices of the differently oriented U.K. and U.S. corporate laws, I must mention two serious errors that permeate the monograph, and some important omissions. First, his thesis is based on his incorrect assertion that the U.S. Constitution prevents enactment of a federal law dealing with corporate governance. Not so. The Commerce Clause allows such laws, and they have been enacted. Indeed, the Securities Exchange Act of 1934 requires that shareholders be given proxy statements prior to voting for directors and for specified events (such as mergers) and requires the publication of periodic financial statements. Over many years, the Securities and Exchange Commission (SEC), which was established by the 1934 act, has promulgated many regulations dealing with corporate governance and shareholders' rights. For example, SEC Rule 12e-3 (1977) annulled a Delaware law that allowed controlling shareholders to take their corporation private without fully compensating minority shareholders, and in 2003 it enacted the "all holders rule" (13e-4 and 14-d-10), which "demolished the specifics of state law on a selective stock buyback" (Roe 2003, 619).¹

¹ Roe offers an extensive examination of the ways that federal law has superseded state laws governing corporate governance. He concludes (pp. 597–598): "If one were only to skim the doctrinal surface, one might think there was a sharp divide between state and federal corporate authority: states govern internal corporate affairs, while the federal rulemakers, via the SEC, govern the external trading of the firm's securities. ... But the internal affairs doctrine is just an understanding, not a crisp constitutional rule, and all corporate law could be federal law." His article provides many examples thereof.

Second, Bush always and only refers to the Securities Act of 1933 as the statute governing financial reporting by publicly traded U.S. corporations. Yet that law deals only with newly issued securities and requirements for prospectuses. He never mentions the Securities Exchange Act of 1934, which governs periodic reporting and includes Regulation S-X, which imposes specific requirements for both prospectuses and periodic reports. Establishment of the SEC was perhaps the most important aspect of the 1934 act that affects financial reports and accounting. The SEC was given the authority to determine how financial reports should be structured and what they should include. Although the SEC has supported and deferred to the public accounting profession, most recently the Financial Accounting Standards Board (FASB), on several occasions it has overruled FASB statements.²

Nor does Bush mention the U.K. Financial Services Authority (FSA), which was created in 1997. The FSA is the first ongoing government authority that could control and enforce financial reporting, although it has not attempted to do so. Previously, the U.K. Companies Act of 1981 only required presentation of financial statements in one of four formats. It was not until 1989 that the Companies Act included a reference to accounting standards. It required companies to state whether these had been followed in preparing the financial statements. U.K. companies must send their financial statements to the Companies House of the Department of Trade and Industry (that he also does not mention), which serves as a public repository for company statements. In the past, the financial reporting provisions of the Companies Act were enforced *only* by the courts. Now they are enforced mainly by the private-sector Financial Reporting Review Panel.

Bush claims that, "Unlike the US Securities Acts of the 1930s, which were introduced to fix a problem, British company law has tended to support good practices, not force new practice or tightly regulate" (p. 5). He is wrong on both counts. Although the Securities Act of 1933 was indeed enacted to "fix a problem," it was not the problem that Bush appears to have in mind. Rather, as Mahoney (2001, 1) documents, the principal impetus and effect of the act was to benefit high-status wholesale investment firms and firms that specialized in retailing by reducing competition from integrated investment firms, new and less known rivals. Inadequate corporate financial disclosure was not dealt with or even considered by the 1933 act.

Unlike the 1933 act, which Mahoney (2001, 1) states, "was drafted in haste and secrecy," the Securities Exchange Act of 1934 was preceded by the U.S. Senate's Stock Exchange Practices ("Pecora") hearings held in 1933 and 1934. After examining the 17 hearing testimony volumes, I found the mention of only one instance of alleged dishonest and deceptive financial reporting (Benston 1969, 517). Furthermore, in 1926 all New York Stock Exchange listed companies provided shareholders with balance sheets and income statements, of which 82 percent were audited by CPAs. All of the balance sheets separated current from noncurrent assets and liabilities. On the income statements, all reported net income, 55 percent revealed sales, 45 percent gave the cost of goods sold, and 71 percent reported depreciation. By 1933, these percentages increased to 62 percent for sales, 53 percent for cost of goods sold, and 93 percent for depreciation; 94 percent of the statements were audited by CPAs (Benston 1969, 519). Thus, the 1934 act was not enacted to "fix a problem," at least not a reporting problem.

The U.K. acts, by contrast, were at times preceded by a scandal, as documented by Rose (1965). For example, the failure of the Royal British Bank in 1856 was followed by the Prevention of Frauds Act of 1857, and the collapse of the City of Glasgow Bank in 1878 was followed by the Companies Act of 1879. Income reporting was changed following disclosure of the use by the Royal Mail case on the use of secret reserves in 1931.

Finally, Bush does not mention the roles played by the London and New York Stock Exchanges (LSE and NYSE, respectively) and the professional accountancy organizations, the ICAEW and the AICPA. In 1933 the NYSE required listed corporations to present audited financial statements to their shareholders, although, in response to the exchange's urging, nearly all did so before this time. As outlined earlier, most of what was required by the SEC was already presented by these corporations.³ Because there is no over-the-counter market in the United Kingdom, the LSE's requirements apply to all publicly traded corporations. These requirements generally have been more extensive and issued earlier than those enacted in the successive Companies Acts. The professional accounting bodies in both countries cooperated with the stock exchanges in codifying accounting rules and auditing procedures. These have been much less detailed in the United Kingdom compared to the United States, but the reason, I suggest, is due more to the litigious environment and the existence of the SEC.

Bush is correct in his repeated basic observation that the U.K. laws, at least at present, are directed toward informing shareholders (earlier the emphasis was on creditors), because the laws apply to all limited liability companies, not only to those with regularly traded securities. He is also correct when he repeatedly notes that U.S. federal laws apply primarily to stock trading rather than specifically to shareholders' rights. However, the periodic

² Statements relating to U.S. and U.K. securities and corporate laws and practices are based on Benston (1976) and Benston et al. (2006, Chapters 4 and 5).

³ In Benston (1973), I report statistical tests that the required disclosure of sales (the most important not previously disclosed item) had no meaningful effect on the stock prices of the affected companies.

disclosure requirements of the 1934 act provide current as well as prospective shareholders with financial statements, and (as noted earlier) the SEC has increasingly become concerned with shareholders' rights. Thus, this is a difference without much meaning.

Bush's key point is that U.K. law directs independent Chartered Accountants (ICAs) to be more concerned with stewardship than with providing investors with information they might use to make investment decisions. He writes: "Under the British governance and reporting model, the end customer and the enforcement mechanism are in each case the body of shareholders" (p. 15). Consequently, "UK auditors are able to report and comment freely in audit opinions, especially if there is uncertainty. A key feature of the US model of financial reporting is that it virtually disallows free-form reporting by an auditor" (p. 27). And: "The UK auditor is a witness for the shareholder interest, not a judge or certifier for market exactitude. Furthermore, unlike in the US, where the share price is of concern, due to their duty of care being different, UK auditors need not concern themselves with the impact of their work on the share price" (p. 35). Rather, "Auditors and directors are jointly and severally liable for actual loss of company property, other than that incurred as part of normal enterprise risk. Auditors are not liable for first loss, that is the directors' responsibility, but negligent failure to detect first loss is the responsibility of the auditor" (p. 37).

In what way, then, is financial reporting different in the United Kingdom compared to the United States as a result of the differences in auditors' responsibilities? U.K. reporting is more principles-based, and U.S. reporting more rules-based (as is well known). But Bush provides few specific examples of how this plays out. Indeed, the only one I could find is an apparent expansion of what a U.K. ICA would report: "The British reporting model *should* treat any self-serving expenditure inefficiency as relevant information for shareholders, ahead of anything that needs to be prescribed for disclosure by additional accounting standards" (p. 15; emphasis added). He implies, but does not come out and say or provide any evidence, that U.K. ICAs report inefficiencies. I am informed by colleagues that this does not occur, although I am unaware of a study of the issue.

Finally, although "economics" and "law" are in the title of his monograph, Bush does not consider the effect of differences in tort law. The United Kingdom's legal system does not allow class action lawsuits, and unsuccessful plaintiffs must pay the defendant's legal costs. This surely affects the willingness of independent auditors to extend their comments beyond those that are legally required. It also explains the bias in the United States toward rules on which companies and auditors can rely to protect themselves from lawsuits. Nor does Bush even discuss economics, as in what effect do differences in financial reporting in the two countries have on returns to shareholders, other investors, and the economies of the countries? Most importantly, he does not consider the effect on accounting standards and financial reporting of the U.S.'s creation of the SEC, an ongoing regulatory agency that has the authority, responsibility, and propensity to make rules and regulations, and compare this situation to the United Kingdom, which, until recently, did not have such an agency. Perhaps I am critical of this omission because I pointed to it as the key difference between the principles-based U.K. system and the more costly rules-based U.S. system in a book comparing the two regimes, also prepared for the ICAEW (Benston 1976). I had hoped that Bush would have updated my analysis, particularly now that the United Kingdom has created the FSA as its regulatory body.

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KEES CAMFFERMAN and STEPHEN A. ZEFF, *Financial Reporting and Global Capital Markets—A History of the International Accounting Standards Committee, 1973–2000* (Oxford, U.K.: Oxford University Press, 2007, ISBN 978-0-19-929629-3, pp. xxiii, 676).*

In the foreword, Sir David Tweedie, chairman of the IASB, describes this book as “the definitive history of the IASC from its inception to its transformation from a part-time to a full-time organization.” This book is certainly that and much more. The authors, Kees Camfferman and Steve Zeff, have done an excellent job of chronicling events at the IASC over its 27-year history. In this review, I summarize the highlights of each chapter and conclude with some personal observations on the contributions of the book.

In Chapter 1, the authors describe the approach they have taken to present the IASC’s history. It contains an overview and synthesis of the major developments organized into three periods: (1) the antecedents and founding of the IASC (Chapters 2–3); (2) the first 14 years of the IASC up to 1987 (Chapters 4–7); and (3) the last 13 years from 1987 to 2000, when the IASC was replaced by the IASB (Chapters 8–13). This chapter contains a summary that, on its own, can be informative for readers new to the world of international accounting and the IASC.

Chapter 2 narrates how the growth of international trade and investment in the post-war period resulted in some of the first calls for internationalism by the accounting profession in the late 1950s and through the 1960s. It identifies Henry Benson, Washington SyCip, and Jacob Kraayenhof as some of the influential voices calling for uniformity or harmonization of accounting and auditing practices worldwide. Some of the significant events it cites in the academic arena are the establishment of international accounting centers at the University of Illinois and the University of Washington, Gerhard Mueller’s doctoral thesis and his research in the New York office of Price Waterhouse, and the launch of *Abacus* at the University of Sydney by Ray Chambers with the specific objective of publishing comparative and international accounting research.

Chapter 3 provides insights into the negotiations that took place on the structure and authority of the IASC, the participating countries and accounting bodies, the funding, and the location. It also discusses the reactions from various national and multilateral organizations to the setting up of the IASC. The authors emphasize Henry Benson’s role “in transforming generally held inchoate feelings about the necessity of international harmonization into a concrete programme of action” (p. 46). This chapter also discusses the start of the controversy over defining the relationship between the IASC and IFAC.

The IASC’s structure, leadership, composition of delegations, and funding during the 1973–1987 period are the subject of Chapter 4. It starts with pen-portraits of the first six chairmen and proceeds to list the composition of the original delegations to the IASC, which were from Australia, Canada, France, Germany, Japan, Mexico, The Netherlands, U.K. and Ireland, and the U.S. Most significantly, the chapter provides the first signs of how some of the early decisions would lead to political controversies later in the IASC’s history. These decisions related to the countries that were included in the process, the composition of the technical staff and the project steering committees, the level and sources of funding, the closed nature of the deliberative process, and the relationship with IFAC.

Chapter 5, “Compromise to Harmonise,” recounts the first phase of the IASC’s standard setting from 1973 to 1987, when it published a total of 31 exposure drafts and two discussion papers, resulting in 26 standards. The authors defend the IASC against the characterization that it was seeking the lowest common denominator in its early standards. They argue that, before 1987, the IASC’s strategy was not to attain uniformity in accounting standards but rather to outlaw the unacceptable and allow the acceptable. The authors explain the multiple options permitted in the early standards as being a result of the board members’ unwillingness to make arbitrary decisions in favor of one particular approach. However, one could argue that it was just as much the political reality of the times that required the IASC to gain more support (and subscribers) for its standards. Given the absence of enforcement backing, one way to gain support was to make the adoption of IAS easy and inexpensive for companies and countries. It was not a coincidence that some of the early subscribers to international standards were developing countries.

During the 1970s and 1980s, the developed countries that were the founding members of the IASC were not the ones that required the use of international standards in their domestic jurisdictions. Chapter 6 reviews “the disappointing performance” of the members and associate members of the IASC during this period. It recounts the IASC’s efforts to gain acceptance of its standards by national regulators and standard-setting bodies and discusses the attitude toward international standards in some 20 countries. Not surprisingly, the longest section is devoted to the United States and the general lack of enthusiasm and even negativity toward the IASC’s work,

* The arrangements for this review were made by Gary L. Sundem, a former Editor of *The Accounting Review*.

particularly at the FASB. The difficulty of the IASC's position was underscored by the fact that, during the same period that it encountered benign neglect from the U.S., the French were unhappy with the IASC because, in their view, "the harmonization sought [was] very much impregnated with American methods and ideas" (p. 168).

Chapter 7 looks at the IASC's efforts to cope with the political challenges it faced in trying to serve the many constituents that had a stake in its work. Among the criticisms was that, while overall, developing countries were more frequent adopters of international standards, their relative lack of inclusion in the process meant that their economic and social circumstances were typically not taken into account by the IASC. The chapter is structured around three interconnected developments: relations with the UN, relations with the OECD, and attempts to bring IASC under the control of IFAC.

Chapter 8 starts Part III of the book. It details the operations of the IASC in the 1987–2000 period, specifically focusing on the people, structure, and funding. This chapter contains several lists of those associated with the IASC, venues, frequency, length of board meetings, and the IASC's revenues and expenses. I found this to be the least interesting chapter in the book. Much of the data contained therein could have simply been included in an appendix.

Chapter 9 contains a discussion of the steps taken by the IASC starting in 1987 to strengthen its standards. In an effort to reduce the options permitted in its standards, the IASC launched the Comparability and Improvements projects, which dominated its agenda from 1987 to 1993. The other important change was that, from 1989 onward, the IASC could refer to its *Framework for the Presentation of Financial Statements* as a basis for deciding which accounting treatments should be eliminated and for developing new standards. The chapter also discusses a number of other projects not directly related to the Comparability and Improvements projects. These include cash flow statements, developing country issues, and bank disclosures.

As mentioned previously, in the late 80s there was a widespread perception and a concern among IASC board members that its standards were mainly being used only in economically developing countries. The board believed that, in order to gain acceptance in economically developed countries, the IASC had to make inroads with securities market regulators, national standard-setters, and major corporations. The IASC decided that the International Organization of Securities Commissions (IOSCO) would be the major focus of its attention to gain acceptance and enforcement backing for its standards. Chapter 10 looks at this effort in two phases—a difficult period from 1987 to 1994 and a more promising phase starting in 1995.

Chapter 11 wraps up the discussion started in Chapter 5 and continued in Chapter 9 on the IASC's efforts to strengthen its standards in order to have them endorsed by IOSCO. It starts with a description of the technical agenda and the standard-setting process and then contains a discussion of several specific standards. The conflicting views of the various participants posed serious challenges to the IASC as it tried to complete the core standards and provide a body of standards for IOSCO's approval. The chapter provides evidence of the growing concern by the Europeans that they were being marginalized and that U.S. GAAP would drive the IASC's standards.

Chapter 12 revisits the question of the acceptance of IASC standards in developed countries and concludes that there was progress made in the 1990s. Specifically, it discusses the responses to the IASC in European countries represented on the IASC board as well as those not on the board; the European Commission's decision to require all listed companies in Europe to apply IASC standards starting in 2005; the support provided by multilateral financial institutions such as the World Bank; and the concerns raised by the muscle-flexing of the group of national standard-setters from the G4 +1.

Chapter 13 provides a detailed inside view of the politically charged process that resulted in the IASC being replaced by a restructured IASB. In a series of meetings from July 1997 to November 1999, the participants went through several iterations of what the new organization would look like in terms of its size, composition, structure, authority, and funding. There were serious fundamental disagreements between the Europeans and the "G4" cadre of standard setters. This chapter provides a blow-by-blow account of the negotiations and how the ultimate decisions were made. The political dynamics at the time of the creation of the IASB remain relevant today, as questions are raised about the direction pursued by the IASB on issues such as the trend toward the greater use of fair values in primary statements (Dilks 2007; Fearnley and Sunder 2007).

This book makes an invaluable contribution by providing a detailed history of the IASC. The authors have painstakingly researched the organization through countless documents and numerous interviews. The result is a narrative that provides as complete a picture of the IASC as we are likely to see. It is a scholarly work that makes for interesting reading for those of us who have a professional interest in the IASC. It is also a fascinating study of the politics of an international organization. In an era of growing multilateralism, the lessons to be learned from Camfferman and Zeff's book extend beyond the arena of accounting standard-setting. They are relevant to a host of other international organizations working to establish international standards. The IASB is to be commended for commissioning and funding this project.

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ROBERT J. KIRSCH, *The International Accounting Standards Committee: A Political History* (Wolters Kluwer (UK) Ltd., 2006, ISBN 978-1-84140-608-4, pp. xx, 465).

When indigenous Australian film maker Rachel Perkins was confronted with the daunting task of telling the story of the first Australians from 1780 to 1992, she received guidance from the celebrated U.S. documentary maker Ken Burns. His advice? Tell it chronologically and through the lives of individuals.⁴ Robert J. Kirsch's history of the IASC is consistent with this approach. It chronicles the development of the idea of the IASC (from 1966), describes the life of the organization from 1973 to 2001, and introduces the key people who shaped the IASC over this period. In the style of historical documentary makers, the author highlights the political forces that shaped the IASC and how these forces were used by its key people to develop the organization throughout its existence.

Kirsch provides a documentary history, using relevant archival material and published documents. In addition, he interviewed and corresponded with most of the surviving IASC Chairmen and Secretaries General as well as a number of Board and staff members. Kirsch was a research fellow at the IASB from July 2001 to August 2002, during which time he gathered archival data and conducted many of the interviews cited in the book.

Covering 465 pages, the book reviews the major milestones in the IASC's existence. Events are explored in detail to explain how they came about and their importance. The depth of discussion will be rewarding for general readers as well as those seeking to explore a particular event or issue in the IASC's history. Kirsch has provided explanations and background to the myriad of events, meshing his material into a narrative which both informs and carries the reader onwards.

Events are covered in a chronological order, with Chapters 2 to 9 addressing particular groupings of years, clustered in terms of a dominant theme. For example, Chapter 2, "The Formative Period: 1966 to 1973," covers the period in which the idea of the IASC developed into reality, describing the people, organizations, and events leading to its formation. Chapter 3, "Challenges, Responses and The First Standards: 1974 to 1977," covers the early years of operation and production of the first standards. Each chapter opens with a list of chapter sections, which give a useful guide to its content. Where appropriate, there is consistency in the way the material is organized within each chapter. For example, Chapters 4 to 8 begin with a discussion of the Chairmen and staff, then lead on to descriptions of issues relating to the organizational structure and relationships with other bodies. Chapters then progress to a discussion of other events in the period, separated into appropriate sections. All chapters conclude with a useful summary of the key events in the period under review.

The history is also analyzed in terms of themes, with 16 noted in Chapter 1. They include the following: how the organization interacted with other entities, responded to its environment, and evolved over time; the importance of the art of politics and compromise in the life of the IASC; the key role of Chairmen and the strategic vision of being the preeminent standard-setting body; and the organization's development as a standard setter, the growth of the board and staff, and the evolution of the standards themselves. This list shows the broad scope of the book. The author uses Chapter 11, "Synthesis," to provide a valuable summary and commentary of the material in the preceding chapters, organized around these themes.

The following quotation comes from a section in Chapter 11 entitled "Diversity and personal relationships." The author provides the material to illustrate cultural diversity, but it also shows how he manages, with humor, to portray the complexity of seeking agreement among people in an international forum:

The investment analysts would want more disclosures; the Swiss would oppose them on the grounds of cost or practicality. The Australians and British want more use of current values; the Germans would oppose this on the grounds of prudence and reliability, and the North Americans on the grounds that it was unclear what

⁴ See George (2007).

exact model was being used to replace historical cost. The European Commission observers could be relied upon to oppose anything not allowed by the Fourth Directive and even to oppose anything that would ban an optional practice in it. The Australians acted as the police for the conceptual framework. The Mexican delegation could be relied upon not to waste the Board's time with idle chatter. The Japanese (quite reasonably) often asked for longer exposure periods for documents, given translation problems. (p. 373)

The book has many attractive features for readers. The author's style is clear and easy to read. All content is carefully referenced, and source materials are quoted in detail. Each chapter features an appropriate use of tables and figures to capture relevant statistical material. It is easy to locate material within chapters, using the chapter index and headings. A surprising weakness of the book is the overall index, which is only two and a half pages long. Given the wealth of detail in the history, the range of events, and the huge cast of characters, a more comprehensive index would have been useful in locating people and topics. There are 14 appendices, providing relevant background material about personnel, committees, meetings, and reproducing constitutional documents.

The author uses many quotations to fill out important aspects of the story. The quoted correspondence and interviews add freshness to the text by allowing the reader to hear the views of key players in the various events, at the time they occurred. For example, David Cairns is quoted about the factors which led to the harmonization project:

the IASC itself recognised that it ought to do something about the choices permitted by its standards. ... When I joined there was already in process a number of reviews of existing standards. ... somehow we developed the idea that we should do it as a single project. (p. 183)

In addition, the author includes vignettes at various points in the chapters. Many controversial events and colorful characters are described in the history, but their nature is often communicated directly through quoted material. For example, the death of Sir Henry Benson, the founding father of the IASC, is recorded in a vignette describing events leading to Michael Sharpe's Chairmanship (1995–97). Just prior to his passing, Benson sent a message to Sharpe saying that he would die happy in the knowledge that the IASC was in good hands.

Lighter material is also included, such as explanations of dining etiquette during sensitive negotiations with the Chinese government about membership on the IASC (vignette, p. 261). Political machinations are constantly illuminated; one notable example is described on page 228. Despite the wish of the Japanese delegation to vote in support of a set of revised, harmonized standards in 1993, they were obliged to follow instructions from Tokyo and abstain, all the while proclaiming support for the work of the IASC. Further embarrassment was caused because the then IASC Chairman, Eiichi Shiratori, was Japanese. Some anecdotes seem unbelievable, even though true; for example, page 234 records that in 1994 Secretary General Cairns agreed to send the SEC a free copy of the bound volume of IASC standards because the SEC had insufficient funds to purchase a copy.

The history is comprehensive and covers important milestones, but must of course reflect the author's views of events. Kirsch had discretion about the extent of coverage given to various events, organizations, people, and relationships. It appears that major events in the IASC's history are covered, but the extent to which particular issues are highlighted reflects the author's view of their importance. If another author were to provide the IASC history, a different view of events and people would be portrayed. While Kirsch appears to present the material in a neutral fashion and to provide source documents which "speak for themselves," the material chosen reflects his perceptions of what is important and interesting. Other participants in the history could well hold a different view of events described.

The book can be read as presented, that is chronologically, or it can be dipped into to suit the reader's purposes. A reader can find material on a particular topic, which can be located and extracted from various chapters. Readers can gather background about people, events, and the standards themselves. For example, Sir David Tweedie is first mentioned on page 76. Then, in Chapters 7, 9, and 10, readers can find out about his involvement in the IASC prior to his appointment as the current Chairman of the IASB. Similarly, those wanting a history of the IASC's relationship with national standard-setters can find relevant discussion in several places in Chapters 4 through 7. The history of specific standards can also be investigated, through summaries in Chapter 9, "Standard Setting from 1995 to 2001," and in other relevant chapters. Interestingly, the history covers not only events at the IASC, but also developments in various countries and organizations that are relevant to the IASC story. Thus, it has a truly international perspective.

The book is aptly titled as a political history. The difficulties of developing an autonomous IASC, obtaining sufficient funding, expanding its influence, and gaining acceptance for its standards make a fascinating political story. Kirsch's book itself reflects the political nature of matters associated with the IASC. The author received financial support from the IASB until September 2002; however, opposition from two important players in the history persuaded the IASB not to proceed with its publication (p. ix). Kirsch does not provide reasons for the withdrawal of support; it could have related to others' views of the selected content or its manner of presentation.

Robert Kirsch is a well-published academic who has written about the IASC and international standard-setting in prior work. In the current book, he creates a valuable record of an important organization, which he describes on page 364 as "the history of the IASC from its conception and birth, through an eventful life, to the end of its first incarnation on 31 March 2001." The organization that led to the current IASB developed in stature and influence over its life, consistent with the goals and aspirations of its chairmen, staff, and supporters. Kirsch's text is a fascinating record of the people and events that shaped the IASC, which will be of interest to students, academics, and practitioners. I recommend this book to all with an interest in the politics of international organizations and those wishing to gain an understanding of current arrangements for setting international accounting standards.

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JAMES OHLSON and ZHAN GAO, *Earnings, Earnings Growth, and Value, in Foundations and Trends® in Accounting*, 1:1 (Boston, MA: now Publishers Inc., 2006, ISBN 1-933019-42-5, pp. ix, 77).

Jim Ohlson and Zhan Gao's monograph is the first in the *Foundations and Trends® in Accounting* series, edited by Stefan J. Reichelstein. This work reviews a valuation model earlier proposed by Ohlson and Juettner-Nauroth (2005) (OJ) and relates it to other valuation models. The monograph is aimed at an academic audience already familiar with the process of assessing stock prices in the light of anticipated streams of earnings and cash distributions. The monograph would be a useful reading in a Ph.D. course covering equity valuation, especially alongside Lundholm and O'Keefe (2001).

The OJ model belongs to a class of valuation models whose members share three assumptions. First, frictions like bankruptcy, taxes, and agency costs are absent. Second, the cost of capital (also known as the normal rate of return) is exogenous and constant over time. Third, future streams of earnings and distributions to shareholders are known with certainty (or, equivalently, that the stock price is equal to a sum of values, each of which is the present value, computed using the normal rate of return, of the expected value of an uncertain future cash flow). The models differ in the state variables they encompass and in the supplemental assumptions made about the evolution of those state variables. In the Residual Income Valuation model, for instance, book value is a state variable, and changes in book value over time are assumed to respect the clean surplus relation. In contrast, the OJ model does not require a book-value state variable. Consequently, the clean surplus relation need not be imposed.

Absent growth, a firm is analogous to a personal savings account. Provided the interest rate does not change, the stream of future deposits and withdrawals from a savings account does not affect its present value, which suggests that the price of a no-growth firm should not be affected by its dividend distribution policy. Ohlson and Gao extend this analogy to growing firms. Along the way, they present many interesting intuitions.

Growth bedevils valuation because the pattern of growth over time may be exceedingly complex. The modeler faces a trade-off between descriptive validity and structural simplicity. In the OJ framework, the overall pattern of growth is assumed to be a function of just two parameters. The first parameter is a measure of growth in the near term. Specifically, the first parameter is the amount by which earnings two years hence exceed a benchmark defined as earnings one year hence plus the normal rate of return accruing in the second year on the first-year increase in shareholders' equity. The second parameter is the rate of growth (assumed to be constant over time) in the first parameter. Thus, the second parameter relates long-term growth to near-term growth. The authors derive comparative statics and offer extensive interpretation of this parsimonious model.

They also explain how the OJ model relates to other models in the class. For instance, the OJ model is a special case of the Abnormal Earnings Generation model. Adding structure to the OJ model by defining a book-value state variable and imposing the clean surplus relation yields the Residual Income Valuation model and the Market-to-Book Valuation formula. As the authors acknowledge, this detailed analysis is somewhat pedantic, but it also increases the value of the monograph as a unifying overview of the many research papers in this area.

Beginning in Chapter 4, the monograph presumes that the reader has the mathematical sophistication to digest statements such as "These two conditions correspond to a standard regularity condition that the maximum root (modulus) of the implied transition matrix ... is strictly less than R" (p. 32). Perseverance is rewarded with insights

into why growth firms' values can be insensitive to dividend policy and how information shocks and accounting conservatism affect P/E ratios and earnings growth. These are neat points. Because I suspect other neat points could be made, I wish the authors had also tackled the valuation effects of taxes, leverage, inflation, and periods of economic contraction (when earnings growth might be negative). A discussion of the sources of variation in the cost of capital across firms or over time also would be illuminating.

The monograph lacks polish. In places, the notation is cumbersome. Jargon and acronyms are used when they might have been avoided. Some important terms do not receive clear definitions. Ohlson and Gao end their work abruptly without a summary or conclusion. Despite these shortcomings, *Earnings, Earnings Growth, and Value* is an informative read.

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GEOFFREY WHITTINGTON, *Profitability, Accounting Theory and Methodology: The Selected Essays of Geoffrey Whittington* (London, U.K.: Routledge, 2007, ISBN 978-0-415-37644-0, pp. x, 464).

Geoffrey Whittington is widely known as an inaugural full-time member of the International Accounting Standards Board (IASB). What is less widely known is how he came to be there. This sampler of his writings traces much of Whittington's intellectual journey, from Junior Research Officer at Cambridge in 1962 to a seat at the IASB table about 40 years later. Whittington's essays constitute the first book in the series, *Routledge Historical Perspectives in Accounting*, which is edited by Stephen A. Zeff. It offers great insight into Whittington's career as a highly respected British academic and an influential regulator.

The book contains a foreword by Zeff, an introduction, six sections, and an index. The introduction consists of an academic autobiography followed by a useful overview of the collection and an explanation of why each essay was selected. When the selections were made, Whittington's output had included more than 50 works (listed pages 21–33), 16 having appeared in *Accounting & Business Research* (ABR), five each in the *Journal of Business Finance and Accounting* (JBFA) and *The British Accounting Review*, and about ten in various economics journals. The collation itself comprises 22 papers published mostly in British journals over almost 30 years. As he explains, they were chosen to give "representative coverage of all my research interests" and biased towards "publication outlets that are less accessible or less well-known to accounting researchers" (Introduction, p. 1).

Section I, dealing with empirical studies based on company accounts, has four papers published between 1972 and 1980, which might be termed Whittington's early period. Two are co-authored, one with his former Ph.D. student, Geoffrey Meeks. During this time, Whittington was involved in constructing what was, for the time, a large-scale electronic database: 360,000 punched cards we are told (p. 3), which on my reckoning was of the order of 200 boxes but would today fit conveniently on a thumb drive. It was compiled from the annual financial statements of the more than 3,000 listed companies in the U.K. and was used to study questions in industrial economics. In broad terms, the database was a "British Compustat," and it was *perhaps* of some regret to Whittington that there was, at the time, no British equivalent of the University of Chicago's CRSP database. I say *perhaps*, because, as Whittington explains on page 6, he was in any case well occupied in the pursuit of questions that did not involve, directly, estimating the market effects of accounting numbers or their disclosure. Two of his conclusions: "take-overs and mergers are largely initiated to create or maintain large companies with relatively poor profitability ... for the benefit of management rather than shareholders" (p. 65); and size is "of overwhelming importance in the explanation of the level of directors' pay" (p. 86). Sound familiar? These papers are noteworthy because they illustrate Whittington's keen interest in industrial economics, his sound appreciation of the vagaries of accounting numbers (e.g., the effect of asset revaluations on performance measures), his clear understanding of the econometrics of the models and estimators he was using (e.g., omitted variables bias, self-selection and look-back survivorship bias, autocorrelation, and when to use "changes on changes" models), the need to demonstrate

robustness of conclusions to alternative measures and models, and especially the difference between “economic significance” and “statistical reliability” (p. 82).

Section II, on the specification of empirical models, contains six papers. The first is a brief comment on a paper by Peasnell and Skerratt, who had provided an “efficient markets” interpretation of an earlier paper he had co-authored with Jones and Tweedie. Even in the late 1970s, Whittington was skeptical of the semi-strong form of the Efficient Market Hypothesis (EMH): it was not “highly plausible” in his view (p. 96), even if it was “fashionable” (p. 94). The second, third, and fourth papers, written in the late 1970s, consider circumstances in which accounting ratios could give reliable signals of economic performance. The fifth is a substantial piece published in 1999 in *JBFA* and co-authored with Tippet, who shares Whittington’s interest in the properties of financial ratios. They explore whether the components of four common ratios (liquid assets and current ratios, and the ratios of inventory to current liabilities and debt to assets) are non-stationary in the statistical sense and, if they are, whether the ratios are also non-stationary. It seemed a nice idea: expressing key variables in ratio form could yield a stationary series. But the evidence gave limited support, illustrating that not all nice ideas work in practice. The sixth and final paper in this section, a 1994 paper on empirical models used in research into the determinants of audit fees, was also published in *JBFA*. Co-authored with Pong, it was initially rejected by a U.S. journal—see the wry dig at the un-named editor on page 14. I am glad that the editor of the British journal showed better judgment, because it is a good paper, making the point well that users of *ad hoc*, single equation models so often found in the accounting and auditing literature can face an identification problem. It is the kind of paper that would be useful even today in a doctoral-level course on research methods.

Section III, on price change accounting, comprises three papers on a subject that commanded Whittington’s attention because of its theoretical challenge. The first paper had the poignant, if somewhat tongue-in-cheek title, “Inflation Accounting—All the Answers.” The title was to be taken literally, it being Whittington’s view that “there are many methods of inflation accounting, each of which is capable of providing an answer (but not necessarily a perfect one) to one or more questions which might be in the minds of users of accounting data” (p. 197). What follows is a delightful exposition of alternative ways of thinking about some of these questions. The second paper, titled “The European Contribution to Inflation Accounting,” was a plenary address to the European Accounting Association in 1983. His aim was “to provide a more balanced picture of the contribution of continental Europe to both the theory and the practice of inflation accounting” (p. 215) and illustrates his bent for history (p. 1). The third paper, “The End of the Current Cost Revolution,” was written with Tweedie, with whom Whittington has had a long association, for Professor Parker’s *Festschrift* in 1997. The paper summarizes the demise of current cost accounting proposals in the U.K., U.S., Australia and New Zealand, Canada, South Africa, The Netherlands, and in some countries in Latin America. One reason given for the demise was reduced demand for adjustments because of lower inflation rates (e.g., we are told on page 258 that, according to the professional accounting body in Argentina, no adjustment is necessary once the annual inflation rate falls below 8 percent). Whittington must have found some comfort in the fact that inflation adjustments remained an issue in regulated industries and the public sector.

Section IV, on taxation and regulation, comprises two papers reflecting Whittington’s experience in income tax reform, as a member of the Meade Committee, which reported in 1978, and when he was a part-time member of the Monopolies Commission from 1987 to 1996. Both papers reflect a lasting interest in distortions caused by inappropriate treatment of inflation and price changes.

Section V, on regulation of accounting and auditing, has four papers that represent Whittington’s involvement with accounting standards. The first paper is a critique of the Dearing and Solomons Reports. The Dearing Committee was asked “to review and make recommendations on the standard-setting process” (as quoted on p. 316). Whittington saw the Dearing Report as a pragmatic, “sensible and practical response” (p. 318) to pressures on the previous system (discussed on pp. 314–315). He was much less enthusiastic about the Solomons Report, which expressed a preference for measurement at “current value, based on Value to the Business” (p. 319), because, to Whittington’s mind, more than one valuation system can have merit. The second paper was co-authored with Tweedie and published in *ABR* in 1990. They identified a set of troublesome accounting problems, including off-balance sheet financing, corporate groups, complex capital transactions, and brands. Tables 1 and 2 contain “a tentative attempt to propose solutions for the problems [they] identified” (p. 336); I would describe the “solutions” as the considered judgments of two thoughtful people well-versed in accounting theory and practice. Little wonder that Tweedie was asked to chair the U.K. Accounting Standards Board and Whittington to be its academic adviser! The third and fourth papers round out the section. The third deals with why and how we should regulate financial reporting, and the fourth, a co-authored paper, ironically is a somewhat disjointed discourse on accounting harmonization in Europe.

The sixth and final section, on surveys and methodology, contains three papers. If by this time the reader has not been persuaded that Whittington is an independently minded academic who “sticks to his guns,” then this section will remove all doubt. The first essay starts off innocuously enough, laying out in a matter-of-fact manner

a scheme for classifying 20th Century accounting research in the English-speaking world. But toward the end of the paper there is a shift of pace, when Whittington quotes approvingly of a 1983 paper in *The Accounting Review* in which Christenson offers a philosophical critique of views espoused by Watts and Zimmerman. That criticism sets the scene for the second paper, a trenchant review of "positive accounting theory." Watts and Zimmerman had posited that the "objective of accounting theory is to *explain* and *predict* accounting practice" (as cited, p. 417), and it clearly offended Whittington mightily. He describes their view as "arrogant" (p. 418), "tendentious" (pp. 417, 418, 423), and audacious (p. 422). In contrast, Whittington would have us believe that he was more tolerant of other approaches to accounting theory (and theorists). And so it seems, until we come to the final paper, "Is Accounting Becoming Too Interesting?" In a humorous but pointed public lecture given in 1995, no hold is barred. Research is characterized (p. 438) as being from the "right" (the Chicago/Rochester School), the center (or "heart," namely the "methods and techniques of accounting" and its "ability to provide a consistent account of economic events and transactions," which obviously interested Whittington the most), and the "left" ("the test of accounting is its effect on society or social relationships").

In sum, any careful reader must be impressed by the sheer quality and practical import of Whittington's work over more than 40 years. Unafraid to express his views or to take issue with the views of others, he does so with solid argument and, where appropriate, more than a dash of British humor. Geoffrey Whittington has rendered great service to academia, to British commerce and industry, and to the accounting world at large; and this collection goes a long way to answering the question, "What maketh the man?" As I put the book down, I found myself wondering what if, instead of answering an advertisement he saw by chance (p. 3) and going to Cambridge, he had joined a doctoral program at a leading North American university? After all, with his native ability, intellectual curiosity and interests in accounting, constructing electronic databases, programming, and econometrics and empirical studies, he would have been a wonderful Ph.D. student wherever he went. Having read the book cover to cover, though, I believe I also know the answer. Had Geoffrey Whittington taken the North American path, the accounting world would have been the poorer for it.

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Western Australia*

Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended December 31, 2006. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s)*.

	Number of Manuscripts	Percent	Cumulative Number	Cumulative Percent
$0 \leq \text{Days} \leq 30$	68	16.23	68	16.23
$31 \leq \text{Days} \leq 60$	173	41.29	241	50.96
$61 \leq \text{Days} \leq 90$	110	26.25	351	83.77
$91 \leq \text{Days} \leq 120$	52	12.41	403	96.18
$121 \leq \text{Days}$	16	3.82	419	100.00
Total	419	100.00		

The mean review time was 58.66; the median review time was 56 days.

New Submissions by calendar year (January 1 to December 31)

1992	241
1993	234
1994	231
1995	195
1996	230
1997	195
1998	196
1999	239
2000	260
2001	260*
2002	324
2003	327
2004	307
2005	389
2006	413

* 2001—in addition to the 260 regular submissions, 68 MSs were submitted to the *TAR* Quality of Earnings Conference.

The acceptance rate (defined as number of MSs accepted divided by the number of new submissions) over the past 5 years is between 12–16% per year.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and associate editors are responsible for providing critically constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation. Associate editors also make editorial recommendations to the editor.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary*.

FORMAT

1. All manuscripts should be printed in 12-point font on one side of 8½ × 11" good quality paper and be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins of at least one inch from top, bottom, and sides should facilitate editing and duplication.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, the author's name, title and affiliation, email address, any acknowledgments, and a footnote indicating whether the author would be willing to share the data (see last paragraph in this statement).

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days*; *3 kilometers*; *30 years*. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis*; *re-form*. See *Webster's* for correct usage.

Keywords: The abstract is to be followed by four keywords that will assist in indexing the paper.

ABSTRACT/INTRODUCTION

An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

TABLES AND FIGURES

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate by marginal notation where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Within-text citations are made using an author-year format. Cited works must correspond to the list of works listed in the "References" section. Authors should make an effort to include the relevant page numbers in the within-text citations.

1. In the text, works are cited as follows: author's last name and year, without comma, in parentheses. For example: one author, (Berry 2003); two authors, (Fehr and Schmidt 2003); three or more authors, (Dechow et al. 1998); more than one work cited, (Cole and Yakushiji 1984; Dechow et al. 1995; Levitt 1998); with two works by the same author(s), (Nelson 2003, 2005).
2. Unless confusion would result, do not use "p." or "pp." before page numbers. For example, (Dechow and Dichev 2002, 41–42).
3. For cited works that include more than one work by an author (or same co-authors) that are published in the same year, the suffix a, b, etc., is to follow the date in the within-text citations and in the "References" section. For example: (Baiman and Rajan 2002a, 2002b).
4. When the author's name is mentioned in the text, it need not be repeated in the citation. For example: "Cohen et al. (2005) provide ..."
5. Citations to institutional works should use acronyms or short titles where practicable. For example: (NCFRR, The Treadway Commission 1987).
6. If the paper refers to statutes, legal treatises, or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a "References" section that contains only those works cited within the text. Each entry should contain all information necessary for unambiguous identification of the published work. Use the following formats (which follow *The Chicago Manual of Style*):

1. Arrange citations in *alphabetical order* according to the surname of the first author or the name of the institution or body responsible for the published work.
2. Arrange citations in *chronological order* when two or more works are by the same first author (regardless of co-authors). Two or more works by the *same author(s)* published in the same year are distinguished by letters a, b, etc., after the year.
3. Use authors' initials instead of proper names.
4. For two or more authors, separate authors with a comma, including a comma before "and" (Dechow, P. M., R. Sloan, and A. Sweeney).
5. Date of publication follows the author's (authors') name(s).
6. Titles of journals or newspapers should not be abbreviated.

Sample entries are as follows:

- Baiman, S., and M. Rajan. 2002a. The role of information and opportunism in the choice of buyer-supplier relationships. *Journal of Accounting Research* 40 (2): 247–278.
- , and ———. 2002b. Incentive issues in inter-firm relationships. *Accounting, Organizations and Society* 27 (3): 213–238.
- Berry, R. 2003. Testimony before the Senate Committee on Homeland Security and Governmental Affairs Permanent Subcommittee on Investigations. November 18. Available at: http://hsgac.senate.gov/_files/111803berry.pdf.
- Cohen, D., A. Dey, and T. Lys. 2005. The Sarbanes Oxley Act of 2002: Implications for compensation structure and risk-taking incentives of CEOs. Working paper, New York University, University of Chicago, and Northwestern University.
- Cole, R., and T. Yakushiji, eds. 1984. *The American and Japanese Auto Industries in Transition*. Ann Arbor, MI: University of Michigan.
- Dechow, P. M., R. Sloan, and A. Sweeney. 1995. Detecting earnings management. *The Accounting Review* 70 (2): 193–225.
- , S. P. Kothari, and R. L. Watts. 1998. The relation between earnings and cash flows. *Journal of Accounting and Economics* 25: 133–168.
- , and I. Dichev. 2002. The quality of accruals and earnings: The role of accrual estimation errors. *The Accounting Review* 77 (Supplement): 35–59.
- Easton, P. 2003. Discussion of: The predictive value of expenses excluded from pro forma earnings. *Review of Accounting Studies* 8: 175–183.
- Engel, E., R. Hayes, and X. Wang. 2007. The Sarbanes-Oxley Act and firms' going-private decisions. *Journal of Accounting and Economics* (forthcoming).
- Financial Accounting Standards Board (FASB). 2006. *Accounting for Uncertainty in Income Taxes, and Interpretation of FASB Statement No. 109*. FASB Interpretation No. 48. Financial Accounting Series. Norwalk, CT: FASB.
- Fehr, E., and K. Schmidt. 2003. A theory of fairness, competition, and cooperation. In *Advances in Behavioral Economics*, edited by C. Camerer, G. Loewenstein, and M. Rabin, 271–296. New York, NY: Princeton University Press.
- Janis, I. L. 1982. *Groupthink: Psychological Studies of Policy Decisions and Fiascoes*. Boston, MA: Houghton Mifflin.
- Levitt, A. 1998. The numbers game. Speech delivered at New York University, Center for Law and Business, September 28.
- Maggi, G. 1999. The value of commitment with imperfect observability and private information. *RAND Journal of Economics* (Winter) 30: 555–574.
- National Commission on Fraudulent Reporting (the Treadway Commission). 1987. *Report of the National Commission on Fraudulent Financial Reporting*. Washington, D.C.: NCFRR.
- Nelson, M. W. 2003. Behavioral evidence on the effects of principles- and rules-based standards. *Accounting Horizons* 17 (1): 91–104.
- . 2005. A review of experimental and archival conflicts-of-interest research in auditing. In *Conflicts of Interest: Challenges and Solutions in Business, Law, Medicine, and Public Policy*, edited by D. A. Moore, D. M. Cain, G. Loewenstein, and M. H. Bazerman. Cambridge, U.K.: Cambridge University Press.
- Rigdon, E. E., R. E. Schumacker, and W. Wothke. 1998. A comparative review of interaction and nonlinear modeling. In *Interaction and Nonlinear Effects in Structural Equation Modeling*, edited by R. E. Schumacker, and G. A. Marcoulides, 1–16. Mahwah, NJ: Erlbaum Associates.
- Schultz, E., and T. Francis. 2002. Companies profit on workers' deaths through "dead peasants" insurance. *Wall Street Journal* (April 19): 1.
- Securities and Exchange Commission (SEC). 2002. *Certification of Disclosure in Companies' Quarterly and Annual Reports*. Release Nos. 33-8124, 34-46427. Washington, D.C.: SEC.
- U.S. House of Representatives. 2002. The Sarbanes-Oxley Act of 2002. Public Law 107-204 [H. R. 3763]. Washington, D.C.: Government Printing Office.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be consecutively numbered throughout the manuscript with superscript Arabic numerals. Footnote text should be doubled-spaced and placed at the end of the article.

SUBMISSION OF MANUSCRIPTS

Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state that the work is not submitted or published elsewhere.
2. For manuscripts reporting on field surveys or experiments: If the additional documentation (e.g., questionnaire, case, interview schedule) is sent as a separate file, then all information that might identify the author(s) must be deleted from the instrument.
3. MS should be submitted via email as Microsoft® Word or PDF files to the TAR office at TARUAZ@email.arizona.edu. A submission letter, a separate cover page, and the MS containing no author identification should be emailed as separate files. The submission fee should be paid online (VISA or MasterCard only) at: <https://aaahq.org/AAAforms/journals/tarsubmit.cfm>. If unable to pay online, a check in U.S. funds, made payable to the American Accounting Association, may be sent to Professor Dan Dhaliwal, Senior Editor, *The Accounting Review*, The University of Arizona, Department of Accounting, McClelland Hall 301, 1130 E. Helen Street, Tucson, AZ 85721-0108. The submission fee in U.S. funds is \$125.00 for members and \$200.00 for nonmembers of the AAA. Effective January 1990, the submission fee is nonrefundable.
4. The author should retain a copy of the paper.
5. Revisions must be submitted within 12 months from request, otherwise they will be considered new submissions.

COMMENTS

Comments on articles previously published in *The Accounting Review* will be reviewed (anonymously) by two reviewers in sequence. The first reviewer will be the author of the original article being subjected to critique. If substance permits, a suitably revised comment will be sent to a second reviewer to determine its publishability in *The Accounting Review*. If a comment is accepted for publication, the original author will be invited to reply. All other editorial requirements, as enumerated above, also apply to proposed comments.

POLICY ON REPRODUCTION

An objective of *The Accounting Review* is to promote the wide dissemination of the results of systematic scholarly inquiries into the broad field of accounting.

Permission is hereby granted to reproduce any of the contents of the *Review* for use in courses of instruction, as long as the source and American Accounting Association copyright are indicated in any such reproductions.

Written application must be made to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, for permission to reproduce any of the contents of the *Review* for use in other than courses of instruction—e.g., inclusion in books of readings or in any other publications intended for general distribution. In consideration for the grant of permission by the *Review* in such instances, the applicant must notify the author(s) in writing of the intended use to be made of each reproduction. Normally, the *Review* will not assess a charge for the waiver of copyright.

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POLICY ON DATA AVAILABILITY

The following policy has been adopted by the Executive Committee in its April 1989 meeting. "An objective of (*The Accounting Review*, *Accounting Horizons*, *Issues in Accounting Education*) is to provide the widest possible dissemination of knowledge based on systematic scholarly inquiries into accounting as a field of professional research, and educational activity. As part of this process, authors are encouraged to make their data available for use by others in extending or replicating results reported in their articles. Authors of articles which report data dependent results should footnote the status of data availability and, when pertinent, this should be accompanied by information on how the data may be obtained."

MARCH 2008 PLACEMENT ADS

The deadline for information to be included in the "Placement Information" section is two months prior to the date of publication. The Review is published in January, March, May, July, and October. Placement advertising and replies should be sent to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, ATTENTION: ADVERTISING DEPT. All replies to box numbers should be on separate sheets to facilitate forwarding.

- 201 UNIVERSITY OF CONNECTICUT, School of Business, Department of Accounting invites applications for a non-tenure-track faculty position at the rank of Instructor in-Residence or Assistant Professor in-Residence, commensurate with the candidate's qualifications, beginning Fall 2008. The successful candidate may be required to teach at both the undergraduate and master's levels. Applicants must have teaching interest in Financial Accounting. However, candidates who can teach in multiple areas are preferred. Salary will be commensurate with candidate credentials and experience. For position requirements and more detailed information, please visit our website: <http://www.hr.uconn.edu/jobs-ntp.html>. Review of applications will begin immediately and continue until the position is filled. Please submit nominations or applications to: Dr. Mohamed E. Hussein, Accounting Department Head, University of Connecticut, School of Business, 2100 Hillside Road, Unit 1041A, Storrs, CT 06269-1041; Email: mohamed.hussein@uconn.edu. The University of Connecticut actively solicits applications from minorities, women, and people with disabilities. (Search # 2008202.)
- 202 UNIVERSITY OF LOUISVILLE, School of Accountancy, invites applications for a tenure-track Assistant Professor beginning August 2008. Applicants must have a Ph.D. or D.B.A. at the date of appointment. A strong commitment to scholarly productivity and teaching experience or interest in financial accounting is an important attribute for the position. Other areas of accounting will also be considered. Salary and teaching loads are highly competitive with other research institutions. Information about the city of Louisville is available at: <http://www.louisvilleky.gov>. The Search Committee will accept applications until a new faculty member is selected. Review of applicants begins December 1, 2007. Candidates must apply for the position online at: <http://www.louisville.edu/jobs>. The Job Opening ID Number for this position is 22158. Contact: William D. Stout, School of Accountancy, College of Business, University of Louisville, Louisville, KY 40292; Email: william.stout@louisville.edu. The University of Louisville is an Equal Opportunity/Affirmative Action Employer.
- 203 UNIVERSITY OF MARYLAND, COLLEGE PARK Dean Opening, Robert H. Smith School of Business. Primary responsibilities of the position include academic leadership, fundraising, maintaining and enhancing external relations, and providing administrative oversight and direction. Candidates should have qualifications for a tenured Full Professorship in one of the School's departments, a distinguished record of scholarly achievement; a strong commitment to excellence in teaching, research, and service; a demonstrated administrative ability to lead a large professional school; an ability to anticipate future challenges and to lead strategic planning and change that contributes to future success; excellent interpersonal and communication skills. See application requirements and complete position description at: <http://www.uhr.umd.edu>. Send materials to: Sandy Davis, Smith School Dean Search Coordinator, 1119 Main Administration Building, University of Maryland, College Park, MD 20742-5031; or electronically to: sandyd@umd.edu. The University of Maryland is an Equal Opportunity Employer. Women and minorities are encouraged to apply.

- 204 EASTERN MICHIGAN UNIVERSITY, Department of Accounting and Finance invites applications for the position of Department Head with desired start date of July 1, 2008. A Ph.D. in Accounting is required along with a record of scholarship and college-level teaching justifying appointment at the tenured Full Professor level in the department. Previous administrative experience, professional certification, and industry work experience are preferred. Nominations are accepted. Applications will be accepted until the position is filled, but reviews will begin immediately. Candidates should send a letter of application and *curriculum vita* listing three references to: Dr. Susan Moeller, Chair, Search Committee, Eastern Michigan University, 406 Owen, Ypsilanti, MI 48197. Email questions to: susan.moeller@emich.edu. EMU is an Affirmative Action/Equal Opportunity Employer.
- 205 DRAKE UNIVERSITY invites applicants for a full-time, tenure-track, Assistant/Associate Professor of Accounting position teaching undergraduate courses in Accounting Information Systems, pending final budget approval. Secondary areas of teaching may include financial, cost/managerial, or information systems. A doctorate in Accounting or Information Systems is preferred, but ABD candidates will be considered. Drake University is a private, comprehensive university dedicated to quality teaching located in Des Moines, Iowa. The College of Business and Public Administration (CBPA) and its School of Accounting are both accredited by AACSB-International. The CBPA offers 10 undergraduate majors, and the Master of Business Administration (M.B.A.), Master of Public Administration (M.P.A.), Master of Financial Management (M.F.M.), and Master of Accounting (M.Acc.). Send application letter and *vita* listing three references to: Professor LouAnn Simpson, School of Accounting, Drake University, 2507 University Avenue, Des Moines, IA 50311; Email: LouAnn.Simpson@drake.edu; Website: <http://www.cbpa.drake.edu>. EEO.
- 206 LAKE ERIE COLLEGE invites applications for a tenure-track Assistant Professor of Accounting beginning Fall 2008. We are seeking candidates that possess a Ph.D. or D.B.A. in Accounting (or ABD status). Instructional responsibilities include teaching undergraduate and graduate accounting courses in auditing, financial, and managerial accounting. Experience teaching, a professional background in accounting, and the strong potential for scholarly activity are important qualifications. Qualified candidates should submit a letter of interest, *curriculum vita*, and three professional references to: Faculty Search, Lake Erie College, 391 W. Washington Street, Human Resources, Box 358, Painesville, OH 44077; or email: humanresources@lec.edu. Review of applications will begin immediately and continue until the position is filled. Questions regarding this position can be directed to: Ms. Julie Ziemak at: ziemak@lec.edu. Visit <http://www.lec.edu> to see the full version of this ad. Lake Erie College is an Equal Opportunity Employer and encourages applications from a diverse constituency.
- 207 AMERICAN UNIVERSITY OF BEIRUT (AUB), Suliman S. Olayan School of Business (OSB) is seeking to fill academic positions at the ranks of Assistant Professor/Associate Professor/Full Professor in Finance and Accounting. Applicants should have a Ph.D. or a D.B.A. Successful candidates will be expected to conduct research at the forefront of their field and to teach at the undergraduate, the M.B.A. and the Executive levels. Appointments will be made either for the Spring 2008 or the Fall 2008–2009 academic year. Salary and benefits packages are internationally competitive. Qualified candidates should submit a letter of application, a current cv, full transcripts, copies of publications, and three letters of recommendation by December 31st (Spring 2008) and January 31st (Fall 2008–09) to: Hala Azar at: ha01@aub.edu.lb For more information: <http://sb.aub.edu.lb/faculty/facultyopenings.asp>.

- 208 LONG ISLAND UNIVERSITY (LIU), Brooklyn Campus invites applications for a tenure-track faculty position in Accounting at the rank of Assistant Professor beginning Fall 2008 in the Department of Accounting, Taxation, and Law. We seek an individual whose scholarly attainments are consistent with tenure-track expectations and who is committed to high-quality teaching, research, and professional service. Candidates must have an earned Ph.D. in Accounting from AACSB-accredited business schools or be near completion of the degree. In addition, candidates with current CPA, CMA, or CIA are preferred. Teaching experience at the undergraduate and/or graduate levels is desired. Application Process: Applications should include *curriculum vitae*, three reference letters, and a job market paper. All material should be sent to: Roberta.Scotto@liu.edu; or send a hard copy to: Search Committee/Accounting Faculty Position, c/o Ms. Roberta Scotto, Department of Accounting, Taxation, and Law, Long Island University, One University Plaza—H700, Brooklyn, NY 11201. For further information, please visit: <http://www.brooklyn.liu.edu>.
- 209 LONG ISLAND UNIVERSITY (LIU) Brooklyn Campus invites applications for a tenure-track faculty position in Business-Law at the rank of Assistant Professor beginning fall 2008 in the Department of Accounting, Taxation, and Law. We seek an individual whose scholarly attainments are consistent with tenure-track expectations and who is committed to high-quality teaching, research, and professional service. Candidates must have an earned doctorate in Business-Law (J.D.) or be near completion of the degree. Teaching experience at the undergraduate and/or graduate levels is desired. Application Process: Applications should include *curriculum vitae*, three reference letters, and a job market paper. All material should be sent to: Roberta.Scotto@liu.edu; or send a hard copy to: Search Committee/Accounting Faculty Position, c/o Ms. Roberta Scotto, Department of Accounting, Taxation, and Law, Long Island University, One University Plaza—H700, Brooklyn, NY 11201. For further information, please visit: <http://www.brooklyn.liu.edu>.
- 210 KOREA ADVANCED INSTITUTE OF SCIENCE & TECHNOLOGY (KAIST) Business School is the premier engineering and science research university in Korea, listed among the top 50 technology universities in the world in 2006 by *Times* magazine. The KAIST Business School, located in Seoul, Korea and accredited by AACSB, was the chair and founding member of the Association of Asia-Pacific Business Schools (AAPBS) and provides leadership and collaboration among the leading business schools in the Asia-Pacific Region. Qualifications, expectations, and application/nomination details for the KAIST Business School deanship can be found at: http://business.kaist.ac.kr/eng/p_about/about.asp?mode=view&ind=141&left_01=05&left_02=05_01. Applications and nominations should be sent to: Dean Search Committee (Attn: Prof. Young-Gul Kim), KAIST Business School, Seoul, Korea; Email: deansearch@business.kaist.ac.kr
- 211 KOREA UNIVERSITY Business School, recognized as the best school in Korea by nationally authorized institutions, seeks applications for faculty positions in Accounting. Rank is open. Review of applications will begin immediately and continue until the positions are filled. We are looking for an outstanding person with evidence of a strong commitment to both teaching and research. Candidates should have proven records consistent with their level of experience and requested rank, including scholarly research, teaching ability, promise of future contributions, and effective interaction with faculty colleagues and students. Salary and benefits are competitive. Please send a cover letter, resume, an unpublished research paper and letter(s) of recommendation to: Professor Seok Woo Jeong, Korea University Business School, C407 LG-Posco Bldg, Seoul, Korea; Email: jeong@korea.ac.kr.

- 212 **SANTA CLARA UNIVERSITY**, Department of Accounting invites applications for a tenure-track faculty position to begin Fall 2008. Rank is open. All research areas will be considered, but priority will be given to applicants with a teaching interest in Managerial/Cost. Candidates for Assistant Professor should have completed or nearly completed their Ph.D. and must demonstrate capacity to undertake scholarly research publishable in leading journals and to provide quality teaching. Candidates for senior positions should demonstrate a substantial research record and evidence of teaching excellence. Preference may be given to applications received by January 15, 2008. Send application letter, resume, research papers, teaching evaluations, and recommendations to: Dr. Yongtae Kim, Department of Accounting, Leavey School of Business, Santa Clara University, Santa Clara, CA 95053-0380; or email: ytkim@scu.edu. SCU is a private university located in Silicon Valley with housing subsidies, extensive research database, and competitive summer support. Visit the website: <http://www.scu.edu/accounting>. SCU is an EEO/AA employer.
- 213 **MONTANA STATE UNIVERSITY-BILLINGS**, College of Business invites applications for a position in Accounting to begin August 2008. Billings has 125,000 people located between rugged mountains and sweeping plains. This is a tenure-track Assistant/Associate/Full Professor level. Teaching assignments include a combination of lower- and upper-division accounting courses. Preferences: Interest in financial accounting, governmental accounting, and/or accounting systems; willingness to work with new technologies in the classroom and in distance delivery; CPA/CMA and relevant work experience; contributions to college's AACSB candidacy process and Northwest accreditation activities. Requirements: Must be "Academically Qualified" to teach in area according to AACSB criteria; Ph.D. or D.B.A. in field; scholarly activity and publications. Review begins 02/05/08 and open until filled. Send letter of application, resume, with three recent reference letters to: Human Resources, MSU-Billings, 1500 N 30th Street, Billings, MT 59101; Phone: (406) 657-2278; Fax (406) 657-2120; Email: employment@msubillings.edu; Web-site: <http://www.msubillings.edu>. AA/EEO
- 214 **NATIONAL UNIVERSITY OF SINGAPORE (NUS)**, Department of Accounting, NUS Business School (<http://www.bschool.nus.edu>), invites applications for accounting tenure-track positions at all ranks (Assistant/Associate/Full) beginning July 2008. All accounting-related disciplines will be considered. Candidates should possess a Ph.D. or be close to completion. Selection for senior ranks will be based on publications and teaching performance. Selection for the junior rank will be based on potential for publications in tier one journals and quality teaching. Quality research is highly encouraged. Our accounting faculty have published in leading journals such as *TAR*, *JAR*, and *CAR*. The school has a wide collection of databases including Compustat, CRSP, Datastream, and I/B/E/S, and provides excellent funding support for research and conferences. Reduced teaching load is available to new hires. We offer competitive salaries, subsidized housing, and educational allowances for staff with children. Please send *cv*, research papers, and three letters of recommendation to: Associate Professor Trevor Wilkins, Coordinator, Recruiting Committee, Department of Accounting, NUS Business School, National University of Singapore, 1 Business Link, Singapore 117592; Email: accbox1@nus.edu.sg; Phone: (65) 6516 7927; Fax: (65) 6779 2083.
- 215 **COLLEGE OF WILLIAM & MARY**, Mason School of Business seeks to hire a faculty member in accounting at the Assistant or Associate Professor level beginning in Fall 2008. Applicants with a primary interest in managerial accounting are especially encouraged, as are those able to teach at both the undergraduate and graduate levels. Applicants with an interest in information systems, auditing, and/or financial accounting will also be considered. More information about the school can be found at: <http://mason.wm.edu/>. See the online job site (<https://jobs.wm.edu>) for a more detailed position description and to submit your application materials. Click on Instructional Faculty and then select the Accounting Faculty Position. Direct any questions to: searches@mason.wm.edu. Materials may also be mailed to the following address: Mason School of Business, College of William & Mary, Attn: Search Coordinator, PO Box 8795, Williamsburg, Virginia 23187-8795. Review begins January 31, 2008 and will continue until an appointment is made. The College is an EO/AA Employer.

- 216 MISSISSIPPI STATE UNIVERSITY, College of Business and Industry, Adkerson School of Accountancy, invites applicants to apply for a tenure-track position with a specialization in tax to begin Fall 2008. Rank is open, but candidates must have an accounting doctorate degree, demonstrated excellence in teaching, and the ability to conduct research leading to publication. Consideration will be given to ABD candidates. In addition, the candidate is expected to participate in professional accounting activities and advise students. Professional certification and accounting experience are highly desirable. Salary is competitive. Send letter of interest, current *curriculum vitae*, and names (including contact information) of three references to: Chair, Search Committee, Adkerson School of Accountancy, Drawer EF, Mississippi State, MS 39762. Also complete the Personal Data Information form online at: <http://www.jobs.msstate.edu>. Position will remain open until filled. MSU is an AA/EOE.
- 217 WEST CHESTER UNIVERSITY (WCU) of Pennsylvania, located approximately 35 miles west of Philadelphia, is seeking applicants for an Assistant/Associate tenure-track faculty position in accounting to start in the Fall 2008 semester. Rank depends upon qualifications. Responsibilities include teaching undergraduate and graduate accounting courses, publishing articles in scholarly journals, and performing service. Qualifications include an earned doctorate degree from an AACSB-accredited institution. Professional experience as well as CPA, CFB, and other professional certifications are a plus. Candidates with an ABD in Accounting will be considered. Send *vita* and a list of references to: Dr. Kevin Flynn, Accounting Search Committee Chair, Department of Accounting, West Chester University, West Chester, PA 19383. Application review will continue until the position is filled. The WCU School of Business is accredited by AACSB International. West Chester University is an Affirmative Action/Equal Opportunity Employer. Women and minorities are encouraged to apply.
- 218 OREGON STATE UNIVERSITY invites applicants for a tenure-track position in Accounting at the rank of Assistant to begin September 2008. Required qualifications include (1) an appropriate earned doctorate from an AACSB-accredited university with an emphasis in accounting; (2) evidence of potential for scholarly publication and teaching effectiveness. Teaching duties will be in any two of the following areas: auditing, financial, or managerial accounting (secondary area can be principles). Professional certification is preferred. Oregon State University is located in Corvallis, a charming city of 53,000 in the heart of the Willamette Valley. Qualified candidates must submit an application electronically including a letter of interest, current *vita*, recent teaching evaluations, evidence of scholarly work, and a list of three references. Further information may be obtained by emailing Cheryl Hoflich (hoflichc@bus.oregonstate.edu). For full consideration, apply by January 31, 2008. To review posting and apply, go to: <http://jobs.oregonstate.edu/applicants/Central?quickFind=51803>. OSU is an AA/EOE.
- 219 CHAPMAN UNIVERSITY, Argyros School of Business invites applications for a tenure-track position for Fall 2008 at the rank of Assistant or Associate Professor. This position offers a competitive salary and teaching load, as well as summer research support. Teaching interests in financial, managerial, and/or auditing are preferred. Applicants must have a doctoral degree or be near completion. Chapman University is located in Orange County, California and has an enrollment of approximately 6,000. The Argyros School is fully accredited by AACSB, additional information can be found at: <http://www.chapman.edu/argyros>. Applicants should submit a cover letter, *vita*, references, and a research paper to Professor Glenn Pfeiffer at: pfeiffer@chapman.edu. Please use "Accounting Faculty Position" in the subject line. Chapman University is an Equal Opportunity Employer, committed to providing career opportunities to all people, without regard to race, color, religion, gender, age, national origin, sexual orientation, disability, or veteran status.

- 219 CHAPMAN UNIVERSITY Argyros School of Business invites applications for a tenure-track position for Fall 2008 at the rank of Assistant or Associate Professor. This position offers a competitive salary and teaching load, as well as summer research support. Teaching interests in financial, managerial, and/or auditing are preferred. Applicants must have a doctoral degree or be near completion. Chapman University is located in Orange County, California and has an enrollment of approximately 6,000. The Argyros School is fully accredited by AACSB, additional information can be found at: <http://www.chapman.edu/argyros>. Applicants should submit a cover letter, *vita*, references, and a research paper to: Professor Glenn Pfeiffer at pfeiffer@chapman.edu. Please use "Accounting Faculty Position" in the subject line. Chapman University is an Equal Opportunity Employer, committed to providing career opportunities to all people, without regard to race, color, religion, gender, age, national origin, sexual orientation, disability, or veteran status.
- 220 LAZAR LEVINE & FELIZ LLP is a leading professional accounting and consulting firm servicing individuals, entities of all kinds, law firms, public corporations, and private companies. Known for providing the highest level of personal, proactive, hands-on client service, our clients experience this commitment to excellence through their direct access to the partnership. Our vision has always been about service. We invite you to benefit from the Lazar Levine & Felix experience where the numbers are what we analyze. Our relationships are what really count. The firm is currently seeking an Author/Technical Editor to assist in writing our very prestigious SEC compliance manual. Ideal for a professor or retirement-minded practitioner. CPA preferred but not required. Salary commensurate with experience. Flexible hours and job location. Please direct inquiries to: Patricia Casey-Olsen, Director of Human Resources, Phone: 646-375-3858; or email: pcasey@lazarcpa.com. Visit: <http://www.lazarcpa.com>.
- 221 MERRIMACK COLLEGE Department of Accounting invites applications for a tenure-track position at the Assistant or Associate Professor level beginning Fall 2008. The Department is seeking an individual holding a doctoral degree in Accounting by the start of the appointment. Candidates must have a commitment to excellence in teaching, research, and professional service. Submit your letter of application, *curriculum vitae*, teaching evaluation, contact information for three references, and recent research publication or defended dissertation to: Sharon LaRoche, Department of Accounting, Box O5, Girard School of Business and International Commerce, Merrimack College, 315 Turnpike Street, North Andover, MA. 01845. Please visit our website at <http://www.merrimack.edu> for additional information.
- 222 MANHATTAN COLLEGE School of Business, an AACSB-accredited school, seeks applicants for a tenure-track position as an Assistant or Associate Professor of Accounting, beginning in August 2008. Candidates must have a Ph.D. or D.B.A. in Accounting as well as evidence of teaching effectiveness and research competence. The Department seeks a candidate willing to teach taxation or audit, but all qualified candidates are encouraged to apply. Manhattan College is an independent Catholic college founded by the De La Salle Christian Brothers. The College is in the Riverdale section of New York City. Salary and rank will be based on qualifications. Please send a letter of application, *vita*, and list of three references to: Mary Michel, Chairperson of Accounting, Law and Computer Information Systems, School of Business, Manhattan College, Riverdale, NY 10471; or email: mary.michel@manhattan.edu. Manhattan College is an Affirmative Action/Equal Opportunity Employer.

- 223 MISSOURI WESTERN STATE UNIVERSITY seeks Accounting Professor (Assistant, Associate or Full) to begin August 2008 (full-time, tenure-track) to teach undergraduate courses in introductory accounting and upper-level courses in one or two of the following areas: auditing, cost/managerial, AIS, taxation; conduct professional development related to accounting and its teaching; advise business students; and provide departmental/university support. Required: Doctorate in accounting required by the time of appointment. Preferred: Teaching experience and applied business experience. MWSU offers varied resources to encourage intellectual contributions and strong collegial atmosphere in a mid-sized city with an arts and history focus, within an hour of the professional sports, music, and theater of Kansas City. Employment is contingent upon proof of the legal right to work in the United States. This proof must be provided prior to consideration/employment at the University. An appointment is not final until proof is provided. Qualified applicants must submit a letter of interest, email address, *curriculum vitae*, unofficial transcripts, a one-page statement of teaching philosophy, and contact information including email addresses for at least three references to: Human Resources, Missouri Western State University, 4525 Downs Drive, St. Joseph, MO 64507, or email: recruitment@missouriwestern.edu. Website: <http://www.missouriwestern.edu>. Review of applications begins immediately and will continue until the position is filled. AA/EOE.
- 224 STATE UNIVERSITY OF NEW YORK AT NEW PALTZ, School of Business invites applications for faculty position in Accounting for Fall 2008. Ph.D. in Accounting required; ABDs near completion encouraged to apply. The School of Business offers six B.S. degrees and M.B.A. degree programs. The University emphasizes a balance between teaching and research. The search will continue until the position is filled. For additional information, please contact: Dr. Hadi Salavitabar, Dean of the School of Business; Phone: (845) 257-2930; Email: salavith@newpaltz.edu; Website: <http://www.newpaltz.edu/schoolofbusiness>. Please send letter of application and supporting documents to the following address or email: Dr. Hadi Salavitabar Dean, School of Business, State University of New York at New Paltz, 1 Hawk Drive, New Paltz, New York, NY 12561; Email: salavith@newpaltz.edu. The State University of New York at New Paltz is an AA/EOE/ADA Employer. Women and minorities are encouraged to apply.
- 225 TOWSON UNIVERSITY has two tenure-track positions in accounting available at the Assistant or Associate Professor level for Fall 2008, contingent upon final budget approval. Competence in financial accounting, cost accounting, and/or auditing is desired. Applicants should possess a Ph.D./D.B.A. in accounting from an AACSB-accredited program, and provide evidence of teaching effectiveness and research productivity or potential. ABDs will be considered if completion is expected within the first year of appointment. The teaching load is nine credit hours per semester. Review of applications begins January 15, 2008. Towson University is AACSB-accredited in both business and accounting, and jointly offers M.B.A. and M.S. in Accounting programs with the University of Baltimore. Interested candidates should submit an application, *vitae*, official transcripts and three letters of reference to: Dr. Andrew Schiff, Recruitment Chair, Towson University, Department of Accounting, Stephens Hall, Room 102, 8000 York Road, Towson, MD 21252-0001; or email: aschiff@towson.edu.

- 226 LASELL COLLEGE, Department of Business Administration seeks applicants for a full-time Assistant Professor of Accountancy. More senior rank will be considered based on experience and other credentials. Responsibilities: Teaching responsibilities include courses in the areas of Accounting Principles, Financial Management, Taxes, and Cost. Supervision of the Volunteer Income Tax Assistance Program is a critical component of this position. Qualifications: An M.B.A., M.S.A., or Ph.D. is required. Certification as a CPA or CMA, prior college teaching experience, and professional experience in accounting are preferred. The successful candidate must be able to work cooperatively and productively with faculty, staff, and students, show evidence of excellent teaching and student advocacy, professional development and productivity, and a commitment to campus service and student advising. Faculty at Lasell are employed on renewable single- and multi-year contracts. Screening of applicants will begin immediately and continue until the position is filled. Candidates should send a cover letter, a philosophy of teaching, resume, and names of three references to: Employment, Lasell College, 1844 Commonwealth Avenue, Newton, MA 02466; Email: employment@lasell.edu; Fax: (617) 243-2105. Minority applicants are strongly encouraged to apply.



Faculty Position in Accounting

Candidates are sought for a tenure track Assistant Professor of Accounting position beginning Fall 2008. Position involves teaching various undergraduate accounting courses. Successful candidates either possess or will soon earn a Ph.D./D.B.A. in Accounting, and show strong commitment to teaching excellence. Review of applications begins February 15 and will continue until position is filled.

Send letter of application (please indicate a: the position you are applying for, and b: where you saw this Ad), resume, contact information of three references, photocopies of transcripts of graduate coursework, and completed Application for Appointment (available at: <http://www.fredonia.edu/humanresources/forms/application.pdf>) to:

Dr. Linda Hall, Chair
Accounting Search Committee
Department of Business Administration
School of Business, E336 Thompson Hall
State University of New York at Fredonia
Fredonia, NY 14063

Fax: (716) 673-3332, E-mail: Linda.Hall@fredonia.edu

SUNY Fredonia is an Affirmative Action/Equal Opportunity employer. We actively seek and encourage applications from minorities, women and people with disabilities.